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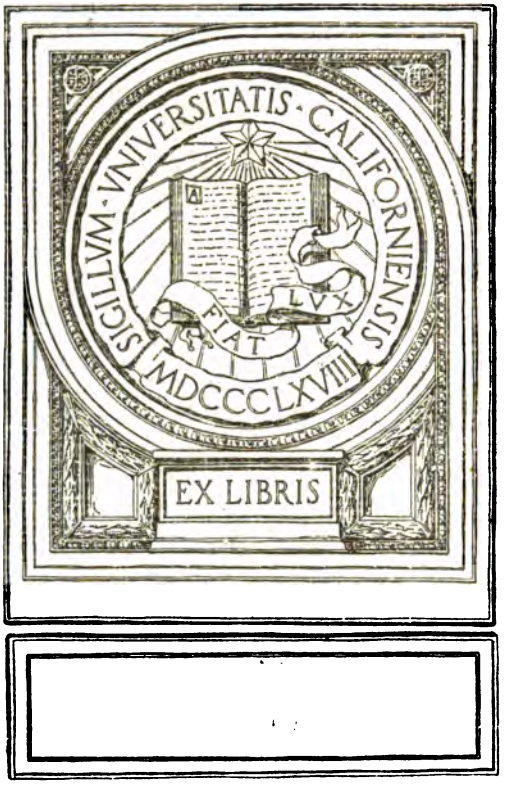

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CHEMISTRY AND ITS BORDERLAND

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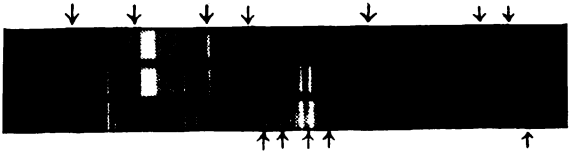
PLATE I

Sodium.

Helium.

SPECTRA OF SODIUM AND HELIUM SHOWING THE CLOSE APPROXIMATION OF TWO LINES.



Carbon.

Copper and carbon.

Copper.

SPECTRA OF CARBON AND COPPER ARCS, AND SPECTRUM OF A MIXTURE OF CARBON AND COPPER.

The arrows indicate lines characteristic of one element and absent from the spectrum of the other, but present in the spectrum of the mixture.

Arc.

Flame.

SPECTRA OF CARBON IN THE ELECTRIC ARC AND IN THE FLAME OF AN ACETYLENE LAMP.

[*Frontispiece.*

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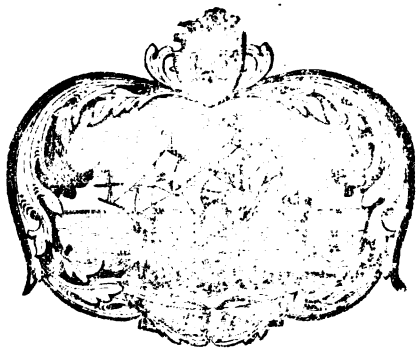
CHEMISTRY AND ITS BORDERLAND

BY

ALFRED W. STEWART, D.Sc.

LECTURER ON ORGANIC CHEMISTRY IN THE QUEEN'S UNIVERSITY OF BELFAST
FORMERLY 1851 EXHIBITION RESEARCH SCHOLAR AND
CARNEGIE RESEARCH FELLOW



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[illegible] ELECTRIC [illegible]
ACETYLENE LAMP.

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FORMERLY 1851 EXHIBITION RESEARCH SCHOLAR AND
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WITH 11 ILLUSTRATIONS AND 2 PLATES

CALIFORNIA

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PREFACE

AT the present time, the sciences and their applications are playing a preponderant part in our daily existence; and yet, so involved are their ramifications that very few people realize the closeness of contact between science and modern life. An interest in the progress of scientific investigation is becoming more marked, however, and a demand is springing up for information upon recent advances in the scientific field.

This demand is easy to supply in certain branches of science; for some lines of scientific thought lend themselves more readily than others to exposition in non-technical language. It is comparatively easy for a biologist or a physicist to give an account of the root principles of his subject such as can readily be understood by the general reader, though the latter may remain ignorant of the minutiæ of the evidence upon which the principles are founded. Chemistry, on the other hand, does not lend itself so well to popular treatment; for this science has evolved a symbolical method of expression of its own which, while to the expert it is more lucid than words, is a stumbling-block to the ordinary man who has not been trained to grasp it. Deprived of its aid, the chemist feels hampered in exposition; and, as a consequence, chemical works designed for the general public are apt to err either in being too elementary in treatment or in taking too much knowledge for granted on the part of the reader.

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In the present volume, it is hoped that these errors have been avoided. No chemical symbols have been employed, and purely elementary questions have not been dealt with at length; so that the non-technical reader will find in the following pages an account of some recent developments in chemistry couched in language which need not present any impediment.

The book may roughly be divided into four sections. In the first of these (Chapters I–III) an attempt has been made to show how chemistry has internally split into various subdivisions, while simultaneously it has expanded into other fields, and has lent the aid of its methods to other sciences and to industry. The second section (Chapters IV–VII) contains chapters on chemical subjects each of which has developed on more or less independent lines. The next section (Chapters VIII–XII) centres round radium and the various problems which it suggests as to the structure of matter. The final section (Chapters XIII–XV) is intended to give some insight into the methods and organization of modern research work, both on the technical and the human side—a field practically unknown to those who are not directly connected with the scientific world.

In conclusion, it may be well to draw attention to another point: the practical and commercial value of pure research. It is very difficult for the non-scientific reader to realize the fact that all the applications of science which render our modern life more comfortable have taken their rise, not in the hands of the applied scientist, but in the laboratories of investigators who were striving after knowledge for its own sake. To take only a single example, the electric lamps of our cities, the electric trains on our railways, the magnetos of our motors and aeroplanes are merely developments of a single experiment made by Faraday in 1831 for a purely scientific purpose. This

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experiment resulted in the discovery of the principle of the electric motor and dynamo; but at the same time when the principle was brought to light no one could have foreseen the vast influence which the new discovery was to exert upon the conditions of social life. Work of the same kind is being carried out in our laboratories to-day, and in the future much of it will no doubt take its place along with Faraday's experiment; but in this country the general public is rather apt to demand immediate dividends and to scoff at the value of pure research work. In the present volume, therefore, it has been thought well to draw attention to a few instances in modern chemical research where investigations which at first sight seemed to be of merely academic interest, have laid the foundations of far-extending industries providing work for thousands of men. These examples might have been multiplied; but the ones which have been selected are sufficient to indicate that money spent on pure research is not by any means wasted, even from the narrowest commercial point of view.

My thanks are due to Dr. H. T. Clarke and Dr. J. E. MacIlwaine, who have assisted me by suggestion and criticism; to Sir William S. McCormick, for supplying me with the statistics of the Carnegie Trust for the Universities of Scotland; to the Secretary to the Royal Commissioners for the Exhibition of 1851, for furnishing me with a copy of the latest Report of the Commissioners; and to Miss F. W. Rea, B.A., B.Sc., and Mr. A. K. Macbeth, M.A., B.Sc., for reading the proofs of the volume.

A. W. S.

THE QUEEN'S UNIVERSITY OF BELFAST,
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CONTENTS

CHAPTER		PAGE
I.	The Ramification of Chemistry	1
II.	The Allies of Chemistry among the Sciences	21
III.	The Relations between Chemistry and Industry	37
IV.	Immuno-Chemistry and some Kindred Problems	51
V.	Colloids and the Ultra-Microscope	82
VI.	The Work of the Spectroscope	98
VII.	Chemistry in Space	118
VIII.	The Inert Gases and their Place among the Elements	135
IX.	Radium	159
X.	Niton	187
XI.	Transmutation	197
XII.	The Nature of the Elements	213
XIII.	Chemical Problems of the Present and Future	226
XIV.	The Methods of Chemical Research	248
XV.	The Organization of Chemical Research	263
APPENDIX A.	A Scheme for the Better Utilization of the Research Capacity of the Country	293
APPENDIX B.	Regulations for Scholars, Fellows, and Grantees of the Carnegie Trust for the Universities of Scotland	299
APPENDIX C.	Regulations for Scholars and Bursars of the Royal Commission for the Exhibition of 1851	304
INDEX		307

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PLATES

PAGE

PLATE I . . . . *Frontispiece*

SPECTRA OF SODIUM AND HELIUM SHOWING THE CLOSE APPROXIMATION OF TWO LINES

SPECTRA OF CARBON AND COPPER ARCS, AND SPECTRUM OF A MIXTURE OF CARBON AND COPPER

SPECTRA OF CARBON IN THE ELECTRIC ARC AND IN THE FLAME OF AN ACETYLENE LAMP

PLATE II . . . . . *facing* 110

AN ABSORPTION SPECTRUM. THE SCALE IS ONE OF WAVE-LENGTHS

FOLDING TABLE . . *at end of volume*

THE PERIODIC ARRANGEMENT OF THE ELEMENTS

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CHEMISTRY AND ITS BORDERLAND

CHAPTER I

THE RAMIFICATION OF CHEMISTRY

WHEN the sciences of the present day are passed in review, the most striking feature which appears to characterize them all is the process of division and subdivision through which they pass. Sciences which, fifty years ago, were treated as unities are now being broken up into sections and subsections, each of which is acquiring a kind of semi-independent status; so that the original science is becoming a mere convenient grouping of the various younger branches. Thus geology has become split up into stratigraphy, which deals with the various formations occurring on the earth's surface; petrology and mineralogy, which are concerned with rocks and their structure; and palæontology, whose subject is fossils. Physics has long been partitioned into a similar series of departments: statics and dynamics, which treat of bodies at rest and in motion; magnetism and electricity; sound, light, and heat; and, in quite recent times, radioactivity. Chemistry has been no exception to this rule: time and again in its history it has thrown out an offshoot which has developed into a semi-independent science; and in this chapter an attempt will be made to indicate the

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different fields which are covered by these subdivisions of the subject.

At the beginning of last century chemical science was approaching a stage of fission, the first of many. A very considerable amount of exact information had been accumulated with regard to the properties and preparation of substances, but the major portion of these data referred to the compounds of metals or to mineral products. Substances such as oil of cloves and turpentine had been investigated, but no means of preparing them artificially had been devised; and it was taken for granted that these vegetable products could only be formed by the intervention of vital action. Animal extracts were also supposed to come into the same category; so that at this period chemistry was divided into two sections: inorganic chemistry, which dealt with materials of a metallic or mineral type, such as iron or lime; and organic chemistry, under which head were included all substances whose formation appeared to be due to vital action.

In 1828, however, this sharp division was broken down by a research of Wöhler on a substance called ammonium cyanate. This body he had prepared by purely "inorganic" methods; but on evaporating its aqueous solution, he found, to his astonishment, that it became transformed into a new body, which he was able to identify as urea. Now, urea is a constituent of human urine, and up to that time it had been regarded as one of the most typical "organic" compounds, the production of which depended on vital action. Yet Wöhler's researches had shown that it could be prepared from purely inorganic materials without any direct vital intervention. This discovery at once swept away the arbitrary division between inorganic and so-called organic derivatives, with the result that chemistry became again unified.

This unity, however, was not destined to remain

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unbroken. The next decades proved that one element stood out above all others in the number of its derivatives: carbon compounds were found to exceed in number all the derivatives of the other elements taken together; and it became clear that a division of chemistry into carbon and non-carbon derivatives would be necessary, both on account of the number of carbon compounds, and also because the properties of all carbon derivatives had much in common, while their differences from the compounds of other elements were strongly marked. An examination of the list of carbon compounds showed that practically all of them lay within the limits of the discarded definition of organic chemistry, as they were for the most part derived from plants or animals; so that it seemed simplest, instead of inventing a new name, to revive the old one of organic chemistry, and include in this field every compound of carbon At a later stage we shall trace the scope of organic chemistry, but first let us glance at the older branch: inorganic, or non-carbon chemistry.

Inorganic chemistry, as we have said, now includes the chemistry of all the compounds which do not contain any carbon. It will be seen that this field is a very varied one, for it comprises all the metallic elements as well as such substances as phosphorus, nitrogen, sulphur, and chlorine. A rough classification of these bodies is required, and the usual one is into metals and non-metals; but this is an arbitrary rather than a correct division, for there are some substances, such as arsenic, which have the general properties of metals, but which, in addition, show other properties resembling those of some non-metallic substances. These intermediate elements are termed metalloids.

Some idea of the problems involved in inorganic chemistry may be obtained if it be assumed that a new element has been found, and that its nature has to be investigated. In the first place, the element must be traced to its source, and the

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chemist must discover where it occurs most plentifully, so as to be sure of a sufficient supply of material for his further investigations. His next step would be to prepare as many compounds of it as he could, and from analyses of these he will be able to draw certain conclusions as to the relationship of the new element with other elements already known. For example, he may find that it forms a chloride which contains one atom of the new element combined with one atom of chlorine. Now, the analyses of the chlorides of other elements show that one atom of sodium combines with one atom of chlorine; one atom of magnesium unites with two atoms of chlorine; whereas an atom of aluminium can attract three atoms of chlorine. It is therefore safe to say that the new element is more akin to sodium than it is to either magnesium or aluminium.

At this point we must be on our guard against a possible mistake. For example, although charcoal and diamond are totally different in their physical properties, yet investigation shows that one atom of the diamond element combines with four atoms of chlorine; and the same is true of the element which forms charcoal. Further examination of the two chlorine derivatives shows them to be identical; this proves that both the diamond and the charcoal are built up from the same element, which turns out to be carbon. Thus carbon is able to exist under various guises—charcoal, diamond, and graphite; yet at first sight these three forms appear to be totally different. This phenomenon is termed allotropy, and when an element appears in more than one physical modification, it is said to exist in allotropic modifications. Thus diamond, graphite, and charcoal are allotropic forms of the one element carbon. Red and yellow phosphorus are examples of the same phenomenon.

To ensure that our hypothetical new element is not a simple allotropic modification of some already known

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element, a series of compounds of the new body must be prepared, and these must be compared with corresponding compounds of the known elements; for the phenomenon of allotropy is confined to the element, and does not persist when the element is transformed into a compound.

Having thus established the individuality of the new body, attention would be turned to determining its physical constants, such as melting-point, specific heat, atomic weight, and so forth; and from the data furnished by all this work an endeavour might be made to allocate a place to the element in the Periodic Arrangement of the elements. The methods by which this can be done will be dealt with in a later chapter,[1] so that we need not enter into them here.

This brief account will give some idea of the processes requisite in the study of inorganic chemistry. Of course, in the case of elements and compounds of commercial importance, further lines of investigation suggest themselves; for methods which are successful in laboratory experiments on a small scale are not always applicable when a process has to be carried out with tons of material instead of ounces. A by-product which is of little account in a laboratory investigation may represent the difference between a paying and a non-paying commercial process; so that in chemical industry we are working under limitations which have to be very carefully investigated before a process can safely be launched as a commercial speculation.

Let us now turn to organic chemistry, which was the first group to segregate itself from the main line of general chemistry. As we have seen, this division between the two allied subjects arose on account of the extraordinary number of carbon derivatives which were discovered; and in order to explain this wonderful preponderance of carbon among compounds we must look more closely into the matter.

[1] See Chapter VIII.

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Non-carbon compounds, as a general rule, contain comparatively few atoms in their molecules, and these atoms are usually of different elements. It is not often that we find many atoms of the same element in an inorganic body. For example, in water there are two atoms of hydrogen and one of oxygen ; in sulphuric acid two atoms of hydrogen are united with one of sulphur and four of oxygen ; in sulphuretted hydrogen two atoms of hydrogen are joined to one of sulphur. Compare this with one of the compounds of carbon and hydrogen which contains no less than sixty carbon atoms united with a hundred and twenty-two hydrogen atoms. The explanation of this difference between the carbon and non-carbon derivatives is to be found in the fact that of all the elements carbon has the greatest power of forming what are called chains of atoms. We can join together in a chain no less that sixty carbon atoms, and this chain will unite with a hundred and twenty-two hydrogen atoms. No other element appears to possess this power in anything approaching this degree, though nitrogen atoms can be joined in short chains of two or three atoms under certain conditions. Thus the carbon atom differs from all other elementary atoms in its power of combining directly with other carbon atoms.

When this was recognized, and when the methods of forming such chains had been worked out in the laboratory, it became a simple matter to produce very complicated carbon derivatives ; and the result has been an enormous outpouring of synthetic material. At the present day, considerably more than a hundred thousand different compounds of carbon are known, and their number is being increased almost daily. Even in the early days of the science the burden of this vast array of compounds made itself felt ; for the author of a large text-book on organic chemistry published in the early part of last century is said to have suggested ironically that the organic chemists of

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that day might take a rest for a few years, as otherwise he despaired of ever making his book complete. At the present time it is almost impossible to keep in touch with the flood of publications which comes out in the various journals; and the Chemical Society publishes monthly a series of abstracts of the most important papers which have appeared, so as to save its members from wading through the details of the originals. Some idea of the bulk of the material may be gathered when it is realized that these condensed abstracts, each of which runs, usually, to merely a few lines, amounted in 1912 to over a thousand pages. Even the Society's "Annual Report on Organic Chemistry," which is supposed to include only the chief work of the year, extends to over a hundred pages of small type.

No one can fail to see that with such a mass of material in our hands we are forced to divide it as far as possible into sections. How can we do this in a logical manner? The problem is really simple. Carbon compounds may be separated into two classes. In the one class, the compound forms a straight chain, each atom following another like links. If, now, the two ends of the chain be joined, a ring will be formed; and certain carbon compounds are known which do actually possess a structure of this type. Thus the whole of the carbon compounds can be classified into two sets: open-chain compounds and cyclic compounds. Then the latter class is divided into two subsections; for the ring may be made up of carbon atoms only, or it may be partly carbon atoms and partly other elements which go to form it. If it is entirely made up of carbon atoms joined together in a circle, then the compound is termed a homocyclic one; whereas if we have in the ring say four carbon and two sulphur atoms, the compound is called a heterocyclic one. Examples of the open-chain type are to be found among the fats and sugars; turpentine, camphor, and such-like aromatic products are

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examples of homocyclic bodies; while the heterocyclic series includes alkaloids like quinine, morphine, and caffeine, as well as more complex substances like the colouring matter of the blood.

The problems before the organic chemist are complex in the extreme, much more so than is the case with inorganic bodies. The subject is a very technical one, and it is almost impossible to give any simple examples of the methods employed; so that we must confine our attention to the most general types of problems.

Since most of the natural products which are used in commerce are derivatives of carbon, it is natural that one of the main features of organic chemical research should be the attempt to discover the way in which these bodies are built up, with a view to producing them synthetically in the laboratory. Let us see if we can gain some insight into the methods employed, while avoiding as far as possible any technical details.

A rough analogy will help us to some extent. Imagine that we were given a watch, and that we were asked to make a replica of it. What would our procedure be? In the first place, we should examine the external part of the watch and take measurements of the case; then we should open the watch and endeavour to learn how it worked. After this, before we could proceed further, we should have to take the machine to pieces and discover what wheels it contained and how they were related to each other. Finally, we should build an exactly similar watch from the data we had acquired. Now the organic chemist, when faced with a new compound, attacks the problem in a somewhat similar manner. In the first place, he submits his crude substance to a laborious process of purification in order to make sure that he is dealing with a single compound and not a mixture of two or more bodies. He then investigates the physical properties of his pure sample; which would correspond to

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our measurements of the watch-case. Next, he tries various reagents upon the substance, with a view to finding out its chemical behaviour as a whole; this would be parallel to our examination of the working of the watch. He then applies different reactions to the compound and endeavours to break it down into simpler substances which he can recognize more easily than the original complex body. This corresponds to our taking the watch to pieces wheel by wheel. From a study of the various decomposition products which he has obtained, the chemist tentatively draws up a plan of the structure of the original compound, and in this formula he expresses by means of symbols all the reactions which the body can exhibit. Finally, in order to be perfectly certain that his plan of the compound's structure is the correct one, he must proceed to synthesize a substance having the structure which he has assigned to the original body. If, when it is prepared, this synthetic substance has all the properties of the natural compound, his task is complete.

This short summary may give a false impression of ease, for, when expressed like this, the task seems an absurdly simple one; but the experimental difficulties in the way are often enormous. Reactions do not always produce good returns of the required material, and much time may have to be spent in finding a way round a difficulty. Another line of attack may have to be chosen after months of labour have been expended; and very often researches in one branch of the subject may have to wait until further light is thrown upon compounds which at first sight appear to have little connection with the point under examination. An example of such a series of researches is to be found in the history of synthetic indigo. The first steps in this work were taken in 1841, but it was not until 1882 that a simple method of synthesizing this body was fully developed; while eight years more were spent in evolving a process suitable to work

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on a manufacturing scale. The results of this research have, however, been momentous; for at the present day synthetic indigo is being put on the market at such a low price that the natural Indian product is practically out of the competition. Thus a steady half-century's pure research has transferred one of the great industries from India to Germany.

To give some idea of the way in which organic chemistry is interlaced with our modern life, we may take a single example: the chemistry of coal-tar. It would probably puzzle the ordinary non-chemical person to give a list of ten substances obtained directly or indirectly from the distillation of coal; but it would assuredly prove a much more formidable problem if we asked an organic chemist to give a complete list of these products, for their number is almost beyond reckoning. Let us deal with the main outlines of the coal-tar industry, refraining from detailed description.

In the early days of the trade, the only products sought for on a large scale were illuminating gas and coke. The former is very largely made up of carbon compounds which are chemically allied to paraffin wax, though the physical properties of the two substances do not in any way suggest such a close relationship. As a by-product in the coal distillation, however, great quantities of tar are produced; and for many years no peculiar use was found for this material apart from its employment as a preservative of wood and so forth. About the middle of last century, however, the study of coal-tar was undertaken by various chemists; and from their researches a perfect maze of industries has sprung up all over the world. In fact, at the present day the value of coal-tar far exceeds that of the illuminating gas which is produced at the same time; and we might almost say that what was originally a mere by-product has now sprung into the chief place in the industry, while

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illuminating gas, once the main object of the distillation, has sunk into a secondary position.

The products of the coal-tar industry are so numerous that no detailed description of them can be attempted here. We must confine ourselves to the history of a single development, and pass over the others with the briefest mention. Before the middle of the last century practically all the dyes in common use were natural products, derived from plants for the most part. In 1856, however, Sir W. H. Perkin, then a boy of eighteen, was testing a process by which he hoped to produce synthetic quinine; and in the course of his work he accidentally obtained a body which seemed worthy of further investigation. His subsequent researches showed that he had produced a synthetic dye, aniline mauve, the first of many similar products which are manufactured at the present day. Now, at the base of this dye lay a substance called benzene; and as the researches upon coal-tar which were then going on had shown that benzene was one of the tar-constituents, the way was opened for a commercial utilization of the by-products of the gas-works. Young Perkin started a factory at Greenford Green for the manufacture of aniline mauve and several similar dyes which, he had discovered, could be prepared by analogous methods. In the history of British technical chemistry the succeeding period is one which must always be looked on with regret. Britain was the country which not only had the greatest supplies of coal-tar, but was also the greatest producer of this material; she had great dye-works, and everything seemed to point to the development of a new industry in which she was not only better equipped than her possible competitors, but in which she actually had a start in the race. It will hardly be credited that such a chance was allowed to slip, but the short-sightedness of her manufacturers was such that this actually occurred. No attempts were made to set the new development going on

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a large scale. Germany stepped into the breach, captured the industry, and has since profited both directly and indirectly from her far-sightedness. For not only did she gain control of the synthetic dyes, but the trained personnel engaged in this industry developed other lines of work, with the result that at the present day she has practically the whole trade of organic technical chemistry in her hands. When we read the following words, in a report on the Exhibition of 1862 made by Hofmann, who was Sir W. H. Perkin's teacher, we cannot but feel that a chance was put before our manufacturers in the plainest manner, though their contempt for scientific chemistry at that time prevented them from appreciating it :—

"England will, beyond question, at no distant day, become herself the greatest colour-producing country in the world; nay, by the strangest of revolutions, she may, ere long, send her coal-derived blues to indigo-growing India, her tar-distilled crimsons to cochineal-producing Mexico, and her fossil substitutes for quercitron and safflower to China, Japan, and other countries whence these articles are now derived."

This revolution has taken place, but it is not England which exports these colouring matters. Within forty years of the first discovery of the coal-tar dyes, Germany's export of these substances had risen to a value of £4,500,000 annually, while ten years later, in 1907, this output had been practically doubled.

Leaving this rather melancholy subject, let us turn to the other products which are obtained in the coal-tar industry, and here we come into touch with many different fields. Medical science utilizes the coal-tar derivatives which organic chemistry has prepared; drugs such as antipyrine, aspirin, saccharin, and antifebrin; antiseptics like carbolic acid and salol; hypnotics such as hypnone. Bacteriology employs the coal-tar colours to stain its

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microbes so as to render them easier to examine. Photography utilizes developers most of which are synthesized from substances found in coal-tar. The perfume industry relies greatly upon the same source; while military science is indebted to organic chemistry and coal-tar for explosives such as lyddite. Future developments may show that in the organic products of coal-distillation we have a source of some hydrocarbon which will help to eke out the supply of petrol for our motors; benzole, which is at present utilized to some extent, is a by-product in gas-manufacture. Many other examples might be quoted; but we have said enough to show how closely the coal-tar industry has become interwoven with our daily life, and how organic chemistry, in the brief space of half a century, has sprung into a commanding position in the community.

The next division of chemistry which comes under our notice is what is termed physical chemistry. To a very great extent, this branch of the science is a product of the last thirty years, though earlier workers had in many cases laid the foundations of certain departments which have since been more fully developed. Physical chemistry, lying as it does between chemistry and physics, embraces the ground common to both these sciences, and its extent is thus very great. We shall confine our survey to a few of the more important sections of the subject.

In the first place, let us take up what are called chemical statics and dynamics. If a flask be half filled with water and closed hermetically by a stopper, it is probable that at the outset the air in the flask will be moderately dry. Gradually, however, some of the water in the flask will evaporate into the contained air, so that this air will become more and more heavily charged with aqueous vapour. Thus there is a continual transference of water-particles from the liquid into the gaseous state. Sooner or later, however, it will be found that the air in the flask does not gain any

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more water-vapour; it has become saturated. In this state, the water and water-vapour in the flask are said to be in equilibrium; and the explanation of this equilibrium is sought in the idea that, although a certain number of particles are still leaving the liquid and becoming gaseous, an equal number of gaseous water-particles are being condensed and are falling back into the liquid. This gives us the conception of equilibrium; and a similar steady interchange has been accepted in the case of two components in a chemical reaction. Now, if the flask of water be heated, the saturation capacity of the air will be increased; more water-particles will be shot up from the surface of the liquid and become gaseous, until finally a new state of equilibrium will be reached in which the interchange of the two sets of particles becomes fixed again. From this it is clear that the position of equilibrium is fixed for a given temperature; but, if the temperature be changed, a new equilibrium is produced in which the relative quantities of water and water-vapour are different from those of the first equilibrium. In other words, all other physical conditions being the same, the equilibrium between water and water-vapour depends upon temperature. The same rule holds good when we take chemical reactions instead of a physical change such as we have chosen in our illustration. The study of problems of this type is the field of chemical statics.

Turn now to an illustration of chemical dynamics. Suppose that we strike a match and watch closely what takes place. At first there is a violent reaction in which the match-tip is very rapidly consumed; then later the wood of the match burns at a slower rate. Clearly, since both phenomena are chemical reactions, we must deduce that all chemical reactions do not proceed with the same velocity. Numerous instances of this kind might be adduced. A train of gunpowder, for example, will burn up more rapidly

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than a slow-match of the same length ; a stream of gas from a jet will catch fire more quickly than a stick of sealing-wax ; and magnesium wire will burn more swiftly than iron wire of the same size. If we were to study such problems as these, we should be entering the field of chemical dynamics. Now, there is one fundamental law in this field which can be very simply explained. It is clear that if two substances are to interact, they must be brought into contact with each other. A match will serve to set fire to gunpowder, but if the match be kept in one room and the gunpowder in another, no reaction will take place between them. Suppose that there were ten heaps of gunpowder in one place and that only one person was available to light them all ; obviously he could only light one at a time, and the ignition of the ten heaps would be a slow process. If two people were employed to light the gunpowder, the ten heaps could be set off in half the time ; while with ten assistants, the whole ten heaps could be lit simultaneously. Now, imagine that instead of the ten heaps of gunpowder we have ten particles of a chemical, and that the taper of the assistant represents another chemical which can react with the gunpowder. It is clear that in a given time only one of the ten particles will react. If we double the amount of the chemical represented by the taper, we shall double the speed of the reaction, for twice the amount of the "gunpowder" chemical will be acted on in unit time. Increase the amount of the "taper" chemical ten times, and the whole of the "gunpowder" chemical will be acted upon in unit time. It will be evident from this illustrative example that the speed of a chemical reaction will depend upon the amounts of the two interacting chemicals which are present in a given space.

A further explanation will make the matter clearer. In a solution, we imagine the dissolved particles to be moving in straight lines, striking the sides of the vessel and rebounding

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again at intervals. Let us call the two interacting substances A and B. Now, in order that a particle of A may interact with a particle of B, it is necessary that they should collide with one another. These collisions will be more frequent when the two sets of particles are plentiful than will be the case if they are only sparsely present in the solution. The case is parallel to that of people walking in a field. If ten boys and ten girls be blindfolded and turned loose in a two-acre field it will be a considerable time before a boy and girl come into touch with each other; but if the number were raised to a thousand boys and a thousand girls, it is clear that the chance of a boy meeting a girl would be very much increased. Similarly, if the number of particles of A and B in a given volume of solution be increased, then the number of collisions between them will rise, and consequently more of A and B will interact in a given time than would be the case if their numbers were kept low. Founded upon this view, we have the Law of Mass Action, according to which the velocity of a reaction depends upon the active masses of the reacting substances present in a given space.

Leaving chemical dynamics at this point, let us look at some other branches of physical chemistry. Since this subject lies on the border between physics and chemistry, a very important part of it is devoted to the study of the physical properties of chemical compounds with a view to tracing the relations between their chemical nature and their physical properties. In this way, the instruments of physics are pressed into the service of chemistry; and we may now point out the applications of one or two of these instruments in the chemical field.

Taking the commonest first, we come to the thermometer. At first sight there does not seem to be a great field before such an instrument as this; but in point of fact it has played a great part on the stage of physical chemistry. In the first place, the criterion of a pure substance in organic

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chemistry is its boiling-point or its melting-point; and as many organic derivatives are utilized in physico-chemical researches, the thermometer proves itself indispensable in these cases. Then, in the study of specific heats, the thermometer is essential. Suppose that we take equal quantities of two substances at the same temperature and impart the same quantity of heat to each. It will be found that one may rise ten degrees in temperature, while the other only increases its temperature by two degrees. Obviously more heat must be communicated to the one than to the other if we are to raise them to the same range of temperature. Water is taken as the standard; and if we find that to raise the temperature of unit mass of a substance through one degree we require thrice the amount of heat which is necessary to raise the temperature of unit mass of water through the same range, we say that the specific heat of the substance is three. The relation between the specific heat and the chemical nature of the substance then becomes one of the problems which physical chemistry has to solve. Similarly, the relation between a body's chemical structure and its boiling-point, melting-point, or heat of formation lie in the domain of physical chemistry.

Another instrument which has been pressed into service by physical chemistry is the refractometer, which serves to determine the refractive index of substances. Here again relations have to be traced between chemical structure and the physical data. The polarimeter also has been utilized, and its employment has led to a great development in organic chemistry, resulting in the advance of our ideas as to chemical structure. Before the polarimeter had done its work, chemical ideas of the nature of compounds were two-dimensional: they bore the same relation to reality that a picture does to the actual object which is represented in it. With the development of stereochemistry, however, this idea fell; and it is now recognized that chemical compounds

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exist in three dimensions, and that it is necessary to take this third dimension into account in some reactions. Before this time only a few cases had been known which could not be explained by two-dimensional formulæ; but as soon as the three-dimensional chemistry, or "Chemistry in Space,"[1] as it was called, developed, research soon brought to light a whole host of substances whose properties could be explained only by means of solid models.

The spectroscope is another weapon which physics has added to the armoury of the chemist; but as this will form the subject-matter of a later chapter,[2] we need not deal with it in this place.

The electric battery also belongs to the class of physical instruments which chemistry has utilized. It is well known that if the two terminals of a battery be connected by means of a copper wire, a current of electricity circulates along the wire, leaving the battery at one pole and returning to it at the other. Now suppose that the wire be broken in the middle and both ends be inserted in a dish of pure water. The current practically ceases to flow. If, however, a drop or two of sulphuric acid be added to the water, the current again pursues its course. The study of phenomena such as these from the chemical side has led to most important results, both from the theoretical standpoint and also from the technical point of view; and the science of electro-chemistry has now taken its place as one of the subdivisions of physical chemistry. Much light has been thrown upon the phenomena of solution in this way.

The microscope has recently been adopted by the chemist in one or two fields. Thus, an examination of the structure of metals and alloys has been carried out by examining microscopically the polished surfaces of slabs of these materials; and much insight into metallic structure has thus been gained. So important has this section of the

[1] See Chapter VII. [2] See Chapter VI.

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subject become that a new offshoot of physical chemistry, termed metallography, has arisen. The microscope has also been combined with the polariscope in the study of a new and puzzling series of substances which came to light a short time ago. These bodies, though liquid, exhibit all the optical phenomena which we associate with crystals; and in consequence of this they have been named "liquid crystals."

We have not sufficient space to enter into a description of the other branches of physical chemistry. Thermodynamics plays a great part in the theoretical side of the subject; and actino-chemistry or photo-chemistry has in recent times made great advances. The chemistry of colloids, which is dealt with later in this volume,[1] might also be classed under the head of physical chemistry, though it is more probable that in the future it will erect itself into a semi-independent science.

Mineralogical chemistry in its more exact stages has been a subject of fairly recent growth. In the early days when the study of this section of chemistry was begun, the chief problems attacked were concerned with the chemical composition of minerals; next came the attempts to prepare synthetic substances similar to the compounds occurring in nature; and at a still later stage we find that the conditions of formation of the natural deposits are being studied with all the methods which physical chemistry provides.

Another branch of chemistry which is rapidly attaining a position of great importance is agricultural chemistry. The study of the conditions in the soil which improve the crops or enable plants to produce the best results is one of the most vital questions from the point of view of the general community. Complementary to this is the science of phyto-chemistry, which deals with the chemical processes

[1] See Chapter V.

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going on in the living plant. Here we enter into what might be called dynamic organic chemistry; and we come into touch with yet another sub-section of the main science: biochemistry, which is really chemistry applied to the study of physiological processes, such as digestion, oxygenation of the blood, secretions, and so forth.

Finally, youngest of all, comes what may be termed radiochemistry, the field of which is confined to the radio-active elements and their transformations. There will be more space to deal with these problems in the later sections of this book.[1]

The various ramifications of the single subject chemistry have now been reviewed; and it will be seen that there is an ever-increasing tendency towards splitting the science into more and more specialized fields. This is an inevitable movement, for at the present time no investigator can hope to do more than keep abreast of the main advances in the general field; unless he concentrates his energies upon some restricted branch, he is unable to master the immense amount of material which is being poured out from other laboratories, and consequently he is not sufficiently in touch with essential details to keep a grip on the advances which are being made by his co-investigators; and without knowledge of this kind it is almost impossible to direct one's researches to the best advantage.

But along with this internal parcelling out of chemistry another movement is taking place at the present time. As will be seen in the next chapter, chemistry is allying itself with other branches of science and bringing its aid to the solution of their problems.

[1] See Chapters IX. and X.

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CHAPTER II

THE ALLIES OF CHEMISTRY AMONG THE SCIENCES

WE have seen how chemistry has been forced by the extent and variety of its problems to undergo a process of what we might term development by fission. Section after section of the science has opened up into new fields; whilst the increase of acquired knowledge has been so great and, in recent times, so rapid as to make it almost an impossibility for any single brain to remember or keep in due perspective the continually increasing mass of experimental and theoretical data which have been gathered in our laboratories.

At the first glance, one is apt to conclude that the only outcome of such a trend in the science must necessarily be the rise of a specialism so limited that the broader problems of chemistry may fall into neglect, owing to there being no workers with an outlook broad enough to see as a whole the main outlines of such questions. On looking into the matter more closely, however, it appears that the movement towards specialization is only one side of the proposition; another line of activity has been opened up which is bringing chemistry into the service of the other sciences. Thus not only is chemistry becoming more highly organized internally, but it is throwing out externally a series of investigations which tend to draw within its sphere of action a whole series of subjects which at one time were considered to be quite beyond chemical treatment. In the present chapter, we may consider some of the more

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important of these fields in which chemistry has come to the aid of cognate sciences and by the application of its special methods has helped to throw light upon some of their problems.

The sciences with which chemistry has come most into touch are astronomy, physics, geology, physiology, and pathology; and though other branches of knowledge have also profited by the experience gained in the chemical laboratory, these five subjects will supply sufficient examples to make clear the value of the aid which the chemist has been able to contribute to its allies.

To non-experts, astronomy must appear to be one of the most unlikely sciences to which chemistry could offer assistance. Apart from the few meteorites which happen to survive their passage through our atmosphere, there is no chance of obtaining actual specimens of the materials from which the extra-terrestrial universe is composed; and it might appear that without such samples no investigation of the composition of our sun and its neighbours could be made. As will be seen in a later chapter,[1] however, physics has forged a weapon which has taken its place in the chemical armoury, and by means of the spectroscope the chemist is able to analyze substances which he has never handled. By combining the spectroscope with the telescope, it is possible to decide whether a given star contains any element known to us; and the same methods have been employed to throw light upon the constitution of certain of the nebulæ.

With regard to physics, the facts adduced in the last chapter showed that the over-lap between physics and chemistry has become in recent years so extensive as to produce the semi-independent science of physical chemistry. Physics and chemistry have gone hand-in-hand in the search for an explanation of the phenomena of solution;

[1] See Chapter VI.

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they have aided one another in the elucidation of problems of refractive index, surface tension, osmotic pressure, and radioactivity, to choose only a few examples from the host of common subjects.

Turning now to geology, we find that chemistry is coming more and more into touch with this science. In earlier days, the sole point of contact between the two subjects lay in the field dealing with the compositions of the minerals from which the earth's crust is built up; and thus the aid brought by chemistry was limited to indications of the chemical nature and crystallographic character of the various ingredients of rock masses. Recently, however, chemistry has opened up a new line in the study of rock formation, and we may briefly indicate the nature of the results which have been obtained.

It will be recalled that one of the foundations of physical chemistry is the idea that under a given set of circumstances we have an equilibrium established. This equilibrium may be altered in certain cases either by a change in physical conditions or by an alteration in the chemical constituents of the system in equilibrium. Now, in the formation of geological strata there are two main processes at work. A fluid rock may gradually crystallize to a solid, as was the case in the production of granite; or there may be a deposition of material from solution, such as gave rise to the various rock-salt beds which are met with in various parts of the world. Both of these processes can be studied in our laboratories, and from the knowledge of the necessary conditions gained in this way, fairly accurate deductions may be drawn as to the conditions under which geological strata have been laid down. With regard to the formation of what are termed igneous rocks—those which were produced by the solidification of fluid magmas—much insight is being gained from the examination of metals and alloys; and further investigations are being carried out upon the

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actual substances which are known to have been produced in the earlier days of the earth's history. This branch of the subject we may leave aside, as it is extremely complicated; but we may deal briefly with the question of the drying up of solutions.

The most famous of these natural beds of salt is found at Stassfurt in Prussia, and it is of a vast extent. If we try to imagine the possible origin of such a mass of salt, the simplest explanation is that the present area of the deposits was at one time the bed of a sea or salt lake which in the course of some geological period became entirely dried up, so that all its dissolved contents were deposited. A parallel process is going on at the present day in the case of the Dead Sea in Palestine, where the water has become quite dense owing to the amount of dissolved material which is brought down into it by rivers.

Now sea-water, in addition to common salt, contains a whole series of other compounds; and it might be anticipated that the bed of such a dried-up inland sea would contain a mixture of all sorts of substances. This is actually the case in the Stassfurt deposits, but the various ingredients are not mixed uniformly together. What actually exists in these beds is an arrangement of the materials into five superimposed layers, each of which is mainly composed of the same mineral. Thus on the top lies a layer of carnallite (a compound of magnesium chloride with potassium chloride); this stratum is about thirty yards in depth, and it contains also a little common salt (sodium chloride). Below this comes a rather thicker bed of a substance called kieserite (magnesium sulphate), and mixed with this there is rather more common salt than was in the upper layer. Deeper still comes a zone of polyhalite (a compound of the sulphates of potassium, magnesium, and calcium). Next comes a bed of anhydrite (calcium sulphate). Finally, under all these, begins the main deposit of rock-salt (see Fig. 1).

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On closer examination of the beds of polyhalite and anhydrite, a curious phenomenon comes to light; for it is found that in each bed there is a series of thin layers of rock-salt interleaved with the layers of the mineral characteristic of the bed. Thus first comes a layer of anhydrite,

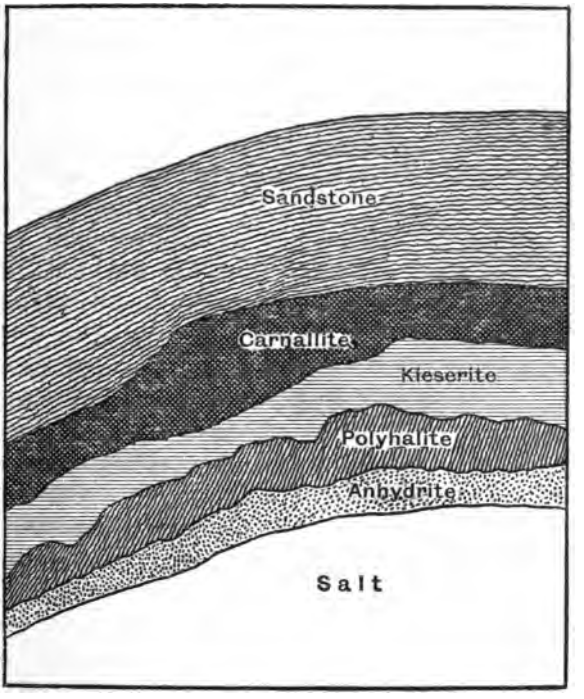

FIG. 1.

then a thin layer of rock-salt, then another layer of anhydrite, and below it another layer of rock-salt. How can we explain this? A plausible solution at once occurs. Every one knows that a lump of sugar will dissolve more easily in hot tea than in cold tea, and this points to the

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fact that a substance such as sugar is less soluble in cold water than in hot water. Now, suppose that sugar be added to a glass of hot water until no more sugar would dissolve; it is evident that when the water cooled, it would not be able to retain all the sugar which it had dissolved, so that some sugar would crystallize out. The same holds good for common salt and water. Clearly, then, there is in these layers of common salt, evidence which points to a change in the temperature conditions of the solution from which the crystals were deposited; at one period the liquid was cold, and salt crystallized out as the water evaporated into the atmosphere; but this was succeeded by a warmer period during which no salt came down from the solution. A careful examination of the salt layers shows that they are fairly regular in their thickness, so that the alternations in temperature must have occurred at regular intervals of time. At once this suggests the alternation of the seasons. During the summer, the temperature of the sea would be warmer than in winter; consequently less salt would be deposited at that time than would be the case when the water was colder. Each thin layer of salt, then, would represent one revolution of the earth about the sun.

So much for that point. We must now turn to the larger question of the four upper beds as a whole, and see how chemistry has thrown light on the conditions which prevailed on that region of the earth's surface at the time when these deposits were laid down. A simple analogy will help to make clear the manner in which this apparently insoluble problem was attacked.

Let us assume that we had two substances, A and B, such that A was a liquid which boiled at 20° C., while B was another liquid which froze at 10° C., and, once frozen, remained solid even if we again raised the temperature. (Of course such a substance as B is purely imaginary.) Now let us suppose that we placed these two bodies in a

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room with a temperature of, say, 15° C. Both would be liquid under these conditions. After leaving them overnight, we come back to the room, and we find the two substances still in the state of liquids. What does this tell us? Clearly during the night the temperature cannot have risen above 20° C. or the whole of the liquid A would have boiled away; and, further, the temperature cannot have fallen below 10° C. or the body B would have frozen to a solid. By a mere inspection of the two substances, then, we could tell that the temperature must have remained during the night between 10° C. and 20° C.

Exactly the same type of reasoning was applied by van 't Hoff to the Stassfurt salt-deposits. Basing his views upon an enormous amount of data which had been laboriously acquired in his laboratory, he was able to point to the fact that certain compounds found in the deposits were only capable of simultaneous deposition from solution within sharply defined limits of temperature. For example, let us take two bodies which actually exist in the Stassfurt beds: Glauber's salt and astrakanite. Glauber's salt, when heated to 18° C., decomposes; while if we cool astrakanite below 4·5° C. it also breaks up. Evidently, then, if we find in the same stratum a mixture of Glauber's salt and astrakanite, we can safely say that the temperature at which that stratum was laid down cannot have been higher than 18° C. nor can it have been lower than 4·5° C. Now, in the Stassfurt beds large numbers of these sensitive compounds are present; and thus it becomes easy to trace very narrow limits for the upper and lower temperatures which were possible during the deposition of the minerals. In some cases, we can be certain even within half a degree. It will be seen that this application of chemistry has furnished what might be termed a "geological thermometer" which makes it possible to determine with very slight errors the actual temperatures of the sea-water when these various strata were laid down;

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and this information is of considerable interest to the geologist, who, without it, would be forced to rely on very rough guesswork.

Turning from geology to physiology, we find that the latter subject has many more points of contact with chemistry than we observed to be the case with geology. Two instances will suffice to show the close relation between the two sciences, and we may select the question of osmotic pressure and the problem of enzyme action as the two most distinctive.

A plant cell or a blood corpuscle may be considered as a skin surrounding a solution of certain salts. Now, further investigation shows that this skin is endowed with a very peculiar property: it will allow water to pass freely through it from the interior to the exterior or *vice versâ*, but it will not permit the passage of any salt. Such a skin as this is termed a semi-permeable membrane, as it is penetrable by water but impenetrable to salt. If such a cell be immersed in a dish of pure water, a most curious phenomenon ensues; for the cell wall becomes ruptured and the cell bursts. On the other hand, if the cell be immersed in a very strong solution of salt, an opposite effect is produced; for the cell wall shrivels up so that the cell becomes contracted, though the membrane remains intact. How can these two results be explained? A simple interpretation is furnished by the assumption that salt has a strong attraction for water. On this view, the first experiment could be explained as follows. When the cell is immersed in pure water, the salt within the cell cannot escape through the cell wall and mix with the water in the dish; yet it is strongly attracted by the water. In order to satisfy this attraction, water from outside the cell is drawn through the membrane, which it can permeate, and flows into the centre of the cell. This inflow of water results in a rise of pressure within the cell, which eventually is unable to withstand it, so that the cell

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wall bursts. Then take the case where the cell is surrounded with a concentrated solution of salt. Here the circumstances of the first experiment are reversed. Inside the cell is a dilute solution of salt; outside it lies a concentrated salt solution. Water will pass through the cell wall from the more dilute solution into the more concentrated one, since the attraction of the salt for water will be greatest when a greater proportion of salt is in the water. Thus water in this case will tend to leave the cell's centre, with the result that the cell, thus deprived of part of its aqueous contents, shrinks together. This explanation of the phenomenon, though not in every respect apposite, will suffice to make the matter fairly clear. The pressure which is produced within the cell by the influx of extra water from outside is called osmotic pressure.

This phenomenon comes into play in physiological processes; and in order to make its importance clear, let us deal with the most important point of all, the question of fertilization.

The ordinary sea-urchin is reproduced by means of eggs which are fertilized by the male. If the egg is not fertilized, but is allowed to lie in sea-water, it dies after a period. Now, Loeb has shown that if an unfertilized egg be placed in a sea-water solution containing any additional salt such as magnesium chloride or potassium chloride which raises the osmotic pressure, an artificial fertilization is produced. Even the addition of sugar or urea to the sea-water has been found efficient as a fertilizing agent. The fertilization proceeds until the new organism has powers of movement. It was at first thought that the fertilizing process might be due to some specific action of the particular chemical added, but further investigation proved that the osmotic pressure of the solution was the controlling influence. Thus we see the close connection between this purely chemical phenomenon and the very foundations of life.

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The examination of another branch of physiological investigation will show how chemical investigations are aiding the kindred science in a totally different field. It has been observed in our laboratories that certain reactions, which ordinarily proceed somewhat slowly, can be hastened if a trace of an apparently indifferent substance be added to the reaction mixture. These indifferent substances are termed catalysts, and in many well-studied cases it seems clear that the small quantity of catalyst present remains unaltered at the end of the experiment. To take one example, which is of importance technically, we may mention the action of spongy platinum upon the interaction of sulphur dioxide and oxygen. If a piece of sulphur be burned, it produces, as every one knows, a gas which has a very characteristic odour, commonly described as the smell of brimstone. This choking gas is what the chemist calls sulphur dioxide. Now, sulphur dioxide, when brought into the presence of oxygen, shows no great avidity to combine with it. Under ordinary conditions, the two gases hardly act upon one another. Bring them together in presence of some spongy platinum, however, and they unite quite readily; and yet no change is observed in the platinum. The platinum in this case plays the part of a catalyst, and enables the sulphur dioxide and oxygen to combine together to yield sulphur trioxide. The technical importance of this reaction lies in the fact that when sulphur trioxide is dissolved in water it produces sulphuric acid, commonly called oil of vitriol, so that by means of the catalytic action of the platinum it is possible to get oil of vitriol from sulphur almost direct, whereas the ordinary process of sulphuric acid manufacture employed previous to the advent of the "contact process" was complicated in the extreme.

Now, an examination of the processes which are taking place in living tissues shows that similar catalysts are at

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work. Reactions which would not proceed under ordinary conditions with any great rapidity are found to be taking place freely in the living animal; and the cause of this apparent abnormality has been traced to the activity of certain catalytic agents which are called enzymes. These enzymes have been isolated from the tissues and have been shown to possess the power of paralleling in a test-tube the reactions which go on in the body. In the earlier days of the subject, it was known that enzyme action was in some ways similar to the fermenting influence of the yeast plant, and it was thought that the enzymes might be considered to be some kind of plants akin to yeast. This idea was disposed of by Buchner, who proved that the yeast plant itself was not the cause of the fermentation which is produced when the fungus is allowed to grow. Buchner was able to demonstrate that even if all organized structure in the yeast be destroyed, the juice of the fungus still had fermenting power; so that fermentation is due to the juice, and not to the plant. The method which he employed was as follows. In the first place, the yeast plants were ground up in a mortar with quartz sand until every trace of organized structure had disappeared. The crushed material was then forced through filters of unglazed porcelain under many atmospheres' pressure, so that all the solid material was retained on the filter and only a liquid could pass through. On examination, this liquid, which clearly could possess no organized structure, was found to possess all the fermenting power of the original plant.

It thus became clear that the fermentation by means of yeast is a catalytic action in which the yeast juice acts as the catalyst; and this view has since been extended to other enzymes which were discovered later. But when we speak of catalytic action we must take care to have a very clear idea in our minds and not to be misled by appearances. If we bring together several chemicals which are capable of

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interacting, we shall find, after the reaction is complete, that equilibrium has been established between the various products; now, if we introduce a catalyst into the mixture at the start, we should find that though the reaction ran its course more rapidly than in the first case, yet at the end of the reaction the same state of equilibrium would be reached as in the first case. The catalyst, then, merely influences the *velocity* of the reaction; it has no effect upon the nature of the products or the course of the reaction. Again, suppose that we took certain bodies which we may call A and B and allowed them to react so as to form some of a new body (A + B). We should find that at the end of the reaction there would be present a little unchanged A and B and some of the newly formed (A + B). These three substances would be in equilibrium, because the compound (A + B) would be decomposing into A and B just as fast as the bodies A and B were uniting to produce more of (A + B). Now, the catalyst has no effect on the relative proportions of the three bodies present when equilibrium is attained, so that clearly if it helps A and B to combine, it must also be helping (A + B) to decompose in the same proportion, as otherwise the equilibrium would be disturbed. From this it is clear that if a catalyst can quicken the velocity of a synthetic or building-up reaction, it can also help the reverse process in the same degree.

From the point of view of physiology, these enzymes are of great importance, for many of the processes of animal metabolism depend upon their presence in the body. Living tissue is in a state of chemical instability; some of it is always being decomposed, while this loss is being steadily compensated by synthetic action. In this state of affairs the enzymes of the living organism play a great part. At the present day we are only on the fringe of the subject, but with further study of the action of these various catalytic agents, it seems probable that we

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may be able to build up by their aid very complicated compounds akin to the natural substances of which the tissues are in great part composed. An example of what is possible at the present day may be given, though it results in fairly simple substances. If an enzyme called maltase be allowed to act upon malt sugar, the malt sugar is broken down into water and a simpler body, grape sugar. This is the disintegrating action of the ferment. Now, if the ferment is allowed to act upon pure water and grape sugar, then some malt sugar is produced. Thus the catalyst can be utilized either as an analytic or a synthetic agent; and when a better understanding of the nature of the enzymes is attained, it seems probable that our synthetic attempts will throw much light upon the nature of the ordinary physiological processes.

We have already spent a considerable amount of space upon the relations of chemistry with these four sciences, so we must pass very briefly over the last section of the subject, its relation to pathology. Here ordinary laboratory methods are brought into play to a very large extent. When certain organs of the body become diseased, or when the system of a patient diverges from the normal, it is usual to find that the secretions of the body alter in character. Thus the amount of urea excreted may be abnormally high or low; the percentage of ammonium salts discharged may be different from that which is found when the patient is in good health; or the stomach contents may diverge from the ordinary standard. In these cases, chemical analysis tells us to what extent the derangement has gone; and it also allows us to control the stages of the disease. For example, take the case of diabetes. Here the patient suddenly begins to excrete sugar, which is not normally the case. Analysis will indicate the amount of sugar liberated, and it will enable the physician to decide whether a given diet is causing harm or good to the

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patient. In such a case, starchy foods are to some extent injurious, as starch is broken down by the enzymes of the body to produce sugar; so that, instead of nourishing the patient, the starchy material is really causing a drain upon the machine's energy. Now, a certain amount of starchy food is desirable, so that what the doctor wishes to find out is the minimum of starch which the patient can absorb without hurt. This can be ascertained by testing the amount of sugar excreted after carefully regulated doses of starchy material, and daily allowance of the starchy foods can be limited to such an extent as to keep within the required minimum.

Sufficient has now been said to show that chemistry has furnished valuable aid to these five sciences, and in this way we are brought face to face with another side of the sciences as a whole. In the last chapter we have seen how the sciences tend inevitably to split up into minor fields of specialization; but side by side with this we find the converse process going on in which one science overlaps the ground of another, bringing its aid in certain special problems and clearing up difficulties by the application of new methods. Thus the sciences are becoming more and more interdependent, and while, on the one hand, there is a tendency towards a narrower specialism, on the other side investigators are making wider surveys. Specialists in one branch are extending their purview to other subjects, so that a continual interweaving of science with science is taking place.

At the root of all this progress lies what may be termed the scientific spirit. Those who have been trained to the methods of scientific investigation in a certain field have acquired not only a grasp of their own particular branch of knowledge, but in addition they have taken on a certain habit of mind and a particular manner of regarding knowledge as a whole. They have come to understand what is

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meant by scientific proof of an hypothesis, and they have learned that even the most fascinating theory may go down into oblivion if it cannot meet new facts. The higher type of scientific intellect, of which Pasteur is a great exemplar, possesses a ccrtain open-mindedness which is rarely met with in other walks of life. In politics or business, a man is dealing with other minds, and he is naturally driven to make out a case which will appeal to these minds rather than to strive after abstract truth. In scientific work, however, there is a sterner ordeal. An investigator knows that sooner or later his work will be tested, perhaps indirectly by investigations on a totally different line, and plausible statement will not help him if his data are erroneous or his hypotheses are unsound. His only safety lies in a single-minded effort to get at the truth so far as it can be attained by his methods; if he fails in this, either wilfully or by over-eagerness, his work may be of interest for a time, but it is sure sooner or later to shipwreck on the rocks of fact. In some cases a gambler may be tempted to run a risk in order to get a short cut to reputation, but the real scientific investigator prefers the slower and surer line of advance.

Again, the scientific training tends to exalt the idea of knowledge attained. Few people outside the scientific circle can grasp the satisfaction felt by an investigator when he feels that his work has opened up a new line of thought. The ordinary man, looking at the progress of chemistry, would be apt to imagine that the synthesis of a new dye, the production of a new explosive or the simplifying of some metallurgical operation would cause a profound impression in the scientific world. But such things can be produced by methods which are largely applications of old principles of no great scientific interest. What is much more rare, and therefore more impressive, is a sudden development, which the ordinary man probably never

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hears mentioned, something which the scientific mind sees will revolutionize more than one field of inquiry and bring a flood of light to bear upon intricate problems still awaiting solution. Applied science any one can understand, as it comes into touch with his daily experience; but applied science is only a superstructure upon the solid foundations which have been laid by men who never sought to make a cash transaction of their knowledge. Yet if it had not been for their labour, divorced apparently from all practical application, applied science would not stand where it does to-day. On the one hand are the needs of humanity, on the other lies the search for knowledge; the bridge between them is applied science. But unless the pursuit of knowledge was going on, there would be no bridge, for the acquirement of knowledge must come before its application. It is the scientific investigator to whom we owe this. He is often handicapped by lack of material; often he is overburdened with uncongenial work which is the price of his researches; as far as social honours go, he is seldom sufficiently wealthy to receive the baronetcy which is within the grasp of many a provincial mayor. But on the other hand, he has some pleasures which the ordinary man cannot share. One is always reminded of the words of Biot, after submitting a research of Pasteur's to a most rigorous test and finding that a line of investigation had been opened up which was undreamed of before, "*Mon cher enfant, j'ai tant aimé les sciences dans ma vie que cela me fait battre le cœur.*"

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CHAPTER III

THE RELATIONS BETWEEN CHEMISTRY AND INDUSTRY

COMPARING a small chemical laboratory with one of our gigantic modern factories, it is hard to believe that the germ of the factory may have emerged from the laboratory; yet in many cases this is a reasonable statement of the facts. It is even more difficult to realize that without the aid of chemistry a city like Johannesburg could never have come into existence; but, as we shall see, the development not only of Johannesburg, but of the whole mining region of the Witwatersrand was conditioned by a laboratory observation. Pure chemistry and the technical side of the subject are closely interlaced at the present day, and the general trend of events points to an even closer relationship between them in the future; so that it may not be without interest to examine a few of the services which the chemist has done for the manufacturer, and thus, indirectly, for the community in general.

The electric furnace supplies us with one of the best examples of the technical developments which may arise from a pure scientific inquiry. Every one is acquainted with the mechanism of the ordinary electric arc lamp, such as is used to light our streets. A current of electricity is allowed to pass across a gap between two carbon electrodes, and the light originates in the arc which is formed at the gap. Now, though part of the electrical energy in the arc is used up

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in yielding light, a very large residuum is converted into heat, so that in the space between the electrodes an extremely high temperature prevails. It occurred to Moissan that this source of heat might be employed to produce combination between various elements which, under other conditions, do not show much avidity for each other, and with this project in view he was led to devise what is now called the electric furnace. In its essentials, the electric furnace is simply a large arc burner; but in order to intensify the effect of the heat generated by the arc, the two electrodes are buried in a block of some non-conducting, infusible substance, such as lime, which prevents any leakage of heat from the furnace. The substance to be treated is placed between the poles of the arc, and the current is then allowed to pass through it. This investigation of Moissan's, at the time it was begun, was a purely scientific undertaking; no particular practical results seemed likely to result from it; but the sequel shows how close is the relation between investigations of so-called "theoretical interest" on the one hand and applied chemistry on the other. Moissan observed that when lime and carbon were heated together in the electric furnace they united to form a substance which is now known as calcium carbide, and this body, when treated with water, evolved the gas which we call acetylene. This result at first sight was of purely scientific interest; but it soon became clear that this "investigation of theoretical interest" had put into our hands a new and most convenient method of obtaining a powerful illuminant. The electric furnace is cheap to manufacture, and if electrical power can be obtained at a reasonable cost, calcium carbide can be economically produced. The raw materials employed are coke and lime, both of which are comparatively inexpensive, and the gas evolved when the carbide is treated with water is of a very high illuminating power. Thus the electric furnace, which twenty years ago appeared to be merely a scientific man's toy, has become the

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agent in the production of one of the most widely used substances whose application is familiar to every class of society.

Acetylene in its turn has been utilized in industrial processes; for if we require an extremely high temperature for welding metals, we can get it by the use of the oxy-acetylene blowpipe, in which oxygen and acetylene are allowed to combine together with an immense evolution of heat. But this by no means exhausts the indirect applications of the electric furnace. We saw that calcium carbide was obtained by subjecting carbon and a calcium compound to the heat of the electric arc. If for the lime we substitute another substance, silica, we obtain silicon carbide as our end-product. This substance has several valuable properties, and it is now manufactured on a large scale under the name of carborundum. It is infusible, even in the heat of the electric arc, and on this account it is useful in the construction of vessels which are to be subjected to very high temperatures. Further, it is an extremely hard substance, so that in the form of powder it is one of the best materials to use in polishing hard surfaces. The carbide of another element, boron, is also distinguished by its extreme hardness, equalling that of the diamond; and it is possible that boron carbide could be employed in the diamond-cutting industry. These few illustrations are sufficient to show the ramifications of the electric furnace industry, and the commercial importance of the subject will be realized when we mention that the annual production of calcium carbide alone is probably more than a hundred thousand tons.

Look now at another branch of chemistry, in which carbon compounds play their part. If carbon be allowed to burn in an unlimited supply of air, a gas is produced which is called carbonic acid gas, or carbon dioxide. This substance, when dissolved in water under pressure, furnishes us with the soda-water of commerce. Suppose, however,

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that the supply of air in which the carbon is allowed to burn be limited, then carbonic acid gas is not the only body produced; along with it another gas, carbon monoxide, is formed. To work with this substance is extremely dangerous; for it is very difficult to detect in the air, since it is tasteless, colourless, and odourless; while, owing to its power of combining with the hæmoglobin of the blood, it exerts a most deleterious action upon animals. The danger of charcoal fires in a room is due to the possibility of carbon monoxide being formed. Now, among the properties of carbon monoxide there is one which is very peculiar. If the gas be passed over certain heated metals such as nickel or iron, a liquid is produced which contains a compound of carbon monoxide with the metal. At first sight this appears to be a fact of purely scientific interest, but further consideration shows that it is of considerable technical importance. Nickel in its natural state usually occurs in conjunction with another metal, cobalt, and the separation of the two is a rather complicated process. But advantage can be taken of the action of carbon monoxide upon nickel to extract the nickel in a very simple manner. The ores are first treated in such a way as to reduce the nickel to a metallic condition, but no attempt is made to extract the free metal. Carbon monoxide is then allowed to act upon the mass at a temperature of about 100° C. under a pressure of about fifteen atmospheres, with the result that the compound of nickel and carbon monoxide is produced. This distils off, and in another part of the apparatus it is heated to 200° C. which decomposes it again into nickel and carbon monoxide. The nickel is deposited, and the carbon monoxide is again passed through the first part of the apparatus, where it again acts upon more nickel, and so on. In this way almost pure nickel is obtained. Thus the laboratory investigation which resulted in the discovery of nickel carbonyl, as the compound is called, has put into our

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hands a very economical method of extracting and purifying nickel on a large scale.

As our third illustration of the development of practical applications from knowledge acquired with a purely scientific aim, we may choose a subject which is familiar to every one. At the present day there are probably few people in our towns who have not seen an incandescent gas mantle, but it is very likely that only an equally small number could give a correct definition of the rare earths from the study of which the incandescent mantle was evolved. Let us take the history of the subject in its main outlines, for it furnishes one of the best examples of the interaction of theory and practice.

To reach the origin of the study of the rare earths, it would be necessary to go back two centuries to the time when some specimens of minerals from Norway, Sweden, and Russia were proved to contain elements that were hitherto unknown. Examination of these bodies showed that they contained certain metallic oxides, yttria and ceria, and further investigations revealed in the same minerals a whole host of other oxides of new elements. In recent years, it has been found that deposits of these substances occur in the Scandinavian peninsula, in the Ural Mountains, and also in some parts of America and Australia, and it appears that they are associated in nature with eruptive rather than sedimentary rocks, granite and other archæan rocks being the usual place to search for them.

In their chemical character, these bodies are marked out from the other elements by their close similarity with each other. It is easy to separate by chemical means two common metals, for example, such as lead and iron; but in the group of the rare earths the elements are so closely allied to each other that a chemical method which affects one of them has an almost exactly similar effect upon the others,

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so that it becomes a very difficult problem to obtain them in a pure condition.

The only simple manner in which they can be separated from each other is by taking advantage of the fact that there are slight differences of solubility between their salts, so that by a process of crystallization many times repeated we are able to free the one salt from its allies. By processes of this type, it has been found possible to separate the rare earth elements from one another; and as the methods are pushed to greater degrees of refinement, it is found that several of the rare earth "elements" (as they were first supposed to be) are really mixtures of two or more fresh elements.

While carrying out some investigations of this kind Auer von Welsbach, about 1880, observed that, when heated in the flame of a Bunsen burner, several of these substances glowed very brilliantly; and in order to study this phenomenon with greater ease, he adopted the plan of saturating a wick of cotton with the salt he was using, and then heating this in the Bunsen flame, which is non-luminous. In this way he was able to observe the luminescence of substances in a convenient manner Further investigations proved that the most brilliant illumination was obtained when a mixture of rare earths was used instead of a pure substance; and, finally, it was found that a mixture of one per cent. of cerium oxide along with ninety-nine per cent. of thorium oxide gave the most brilliant results.

By 1891 the incandescent mantle had become a practical possibility. The liability of the mantle to crumple and become weakened before use was obviated by soaking it in collodion, which stiffened it, while the collodion film could be burned off as soon as the mantle was placed in position on the burner.

But at this point the fortunes of the new industry hung in the balance. Though the supply of rare earths had been

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amply sufficient for the purposes of scientific investigation, it seemed doubtful whether this supply would be able to meet the demands which would be made upon it when the incandescent mantle became a commercial success. At the time when the new mode of illumination was placed upon the market, the mineral upon which it depended cost over a sovereign an ounce, and, what was even more grave, it could only be obtained in small amounts, twenty or thirty pounds being a very fair find at one time.

The demand for incandescent mantles soon made it clear that other sources of supply must be found, if the process was to keep the position it had gained, and a search for other deposits was commenced. It was recalled that thorium had been found in certain abandoned alluvial gold claims in Carolina, and soon the monazite sand of these districts was found to be a new mine of rare earths. Ceylon also yielded a material called thorianite, which is very rich in oxide of thorium. Within ten years, owing to these discoveries, the price of the rare earths dropped to two or three per cent. of its original amount, and at the present day we can buy thorium oxide, for example, for about forty shillings a pound. The permanence of the incandescent mantle industry, as far as this point is concerned, seems to be assured for many years, as the deposits recently discovered are very extensive.

But this does not bring us to the end of the advantages which the study of the rare earths has secured for us. Further investigation of these bodies led to the isolation of the elements themselves from the oxides found in nature; and, in the case of the element cerium, this research brought out the possibility of further practical uses for the rare earths. It was observed that when metallic cerium is scratched, very little friction suffices to make it throw off very hot sparks. This suggested that it might be possible to revive the old flint tinder-box under a new form, utilizing

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cerium instead of steel. But here a practical difficulty arose, for cerium is a very easily oxidized substance; it rusts with the greatest readiness, and when rusted it will yield no sparks. A study of the alloys of the metal gave the clue to a way out of this *impasse*. An iron-cerium alloy when submitted to friction yields torrents of sparks; it does not rust so easily as cerium; and it has the further advantage of superior hardness, for pure cerium was found to be too soft to withstand much rubbing. It is unnecessary to trace the commercial results of this work; every one is acquainted with the cerium alloy which is now widely used for gas-lighters, pocket tinder-boxes, and such purposes.

So much for the assistance which science has lent to industry in this field. But it would be leaving half the story untold if we did not point out the advantage which science has reaped from this commercial exploitation. Before the rise of the Welsbach mantle, the rare earths were very difficult to obtain, and hence their investigation was carried out under adverse circumstances with very minute quantities of material. Since the development of the monazite deposits comparatively large amounts of minerals have been collected, and as most of these are mere by-products from the point of view of the mantle-factories, it is now possible for the scientific chemist to obtain an almost unlimited supply of these interesting bodies for his experiments. Thus the progress of science and industry in this case has been a joint one; and if one branch has received aid, it has helped the other in its turn.

Let us now turn to an industrial chemical process which lies at the centre of our modern system of commercial exchange. Gold differs from most other metals in that its ore, instead of being an oxide or a salt of the element, is the metal itself, and this peculiarity it owes to the fact that chemically it is but slightly reactive. It does not oxidize on

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exposure to the air, so that metallic gold is not liable to become converted into a compound by ordinary natural processes. Now, since gold occurs in nature in the metallic form, the method of extraction which we employ will depend upon the size of the masses in which it is found. In cases where it is found as nuggets among alluvial gravels, it is extracted, as most people know, by a simple process of washing. The gravel being light compared with the gold, the latter is left at the bottom of a vessel in which the gravel is washed by means of running water. When gold is found in the form of veins in quartz, however, this method cannot be directly employed; for the rock must first be crushed into a workable form, and in the course of this process the gold is often reduced to very fine particles which are too light to be easily dealt with by the washing method. In such a case as this, another method has to be brought into play. The sludge of crushed rock is allowed to pass over mercury; and as this substance has the power of forming an amalgam with gold, the useless rock detritus passes off, leaving the gold in amalgamated form in the mercury, from which it can be recovered by a process of distillation.

These two methods, however, are too rough and ready to trap all the gold; and when we come to a quartz which is not rich in gold, we should find that the process would not pay its way owing to the involuntary losses in the recovery methods. In the mines of the Witwatersrand, for example, the amount of gold in the rock only amounts on the average to nine pennyweights in each two thousand pounds of quartz, and only by the most efficient methods of extraction can the mines be run at a profit. To attempt to recover the gold by mere washing would be hopeless; even the amalgamation process is too uneconomical; so recourse was had to a purely chemical method.

Three-quarters of a century ago, it was noticed in laboratory practice that a solution of potassium cyanide had the

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property of dissolving gold, and from this observation has sprung the major part of the supremacy of the South African mines in the world's markets at the present day. If the gold-bearing quartz be crushed very finely and the particles be then steeped in a solution of potassium cyanide, a solution of the greater part of the metal can be obtained. In order to extract the gold from the liquid, all that is necessary is to throw in some shavings of zinc. The zinc in turn goes into solution, and in its place the gold is deposited. Or, instead of using zinc, an electric current can be passed through the solution, and in this way the gold can be extracted at one of the electrodes.

Taking a typical case, the figures will show the immense utility of this process of extraction. The amount of gold present in the rock may be taken as 9 dwt. per 2000 lbs. The amalgamation process is first employed, and by its aid 5½ dwt. of the gold can be removed. The residues from this process are called tailings, and they contain, of course, about 3½ dwt. of gold. The cyanide process is now called into play, and by means of it a further 2 dwt. can be extracted from these tailings. In other words, by means of the cyanide process the total extraction of gold has been increased by thirty per cent., and this difference in the case of many mines represents the margin between running at a loss and making a good profit.

It would occupy too much space were we to go into details of the other processes which have been evolved from data originally gathered with no thought of their technical application. Examples can be found in almost every branch of applied chemistry. In quite recent times, the commercial working of the Stassfurt salt deposits has been placed upon a much better footing by the labours of van 't Hoff in the field of geological chemistry; the immense fabric of German technical chemistry owes its existence to an attempt to synthesize quinine in the laboratory; the

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metallurgy of iron has been more scientifically dealt with since the rise of physical chemistry; the manufacture of sulphuric acid has been simplified by the study of catalysis; and the explosives so widely utilized in mining and engineering are simply adaptations of pure research work.

So far, in this chapter, we have dealt with one section of a problem which is really twofold. When chemistry is called in to aid industrial practice, the task before it may lie in either of two fields. The chemist may be asked to discover a totally new method of carrying out some required operation, and the foregoing paragraphs have shown how this demand has been responded to in certain cases. But on the other hand, the question to be solved may be of a different type, for the chemist may be asked to take an empirical process already in existence, and, after studying it scientifically, to suggest methods by means of which it may be improved.

For example, celluloid is a widely used substance, but it has the very grave drawback of being highly inflammable. This was not of so much consequence until quite recently, except in the cases where the material was employed for combs and such objects of personal use which might be apt to catch fire. Photographic films, at first, did not seem to call for any special precaution, and celluloid was generally employed in their manufacture. Then came the cinematograph, and with it the inflammable film became a public danger. Instead of the risk being limited to a person here and there burning his fingers, the flare-up of a cinematograph film might involve a whole theatre full of people in panic with very serious results. It became necessary to devise some method by which the good qualities of celluloid could be retained, while the films were robbed of their high inflammability. The chemist was set to work upon this problem, and in a very short time films of much less inflammable character were produced.

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An example of a similar type, though upon a much larger scale, is to be found in the sugar industry. In chemistry the name "sugar" does not indicate one particular substance, but is used as a general term for a series of allied compounds. For commercial purposes one of these bodies, ordinary cane-sugar, is of most importance; though fruit-sugar and grape-sugar are also products of marketable value. Cane-sugar is found in almost all plants, for it serves the vegetable kingdom as a kind of reserve material just as glycogen (animal starch) acts as a store of excess nourishment in animals. Now, from the point of view of commercial processes two factors come into play: a plant must be found which contains a percentage of sugar in its sap which is high enough to make its extraction worth while; and, secondly, plants must be selected which will permit of the sugar being cheaply removed from the sap. For a very considerable period, the sugar-cane was the main source of our sugar supply, though maple sugar and palm sugar were also utilized; but in more recent times attempts have been made to extend our resources in this direction.

An examination of the problem of sugar extraction from plants shows that a certain class of vegetation is barred out upon technical grounds. Among the class of what the chemist calls sugars are certain substances which, under the action of slaked lime, undergo various chemical changes, and the resulting products are undesirable impurities in commercial cane-sugar. Plants which are rich in these so-called "reducing sugars" are therefore of very inferior value as sources of cane-sugar for our markets. In this way, we are forced to rule out of consideration practically all plants with two exceptions: the sugar-cane and the beet-root. If asked to choose between these two as a source of sugar, the ordinary person would probably have no hesitation in choosing the sugar-cane, but in view of the amount of reducing sugars in each of them, the beet-root actually is

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a better commercial product. Sugar-cane contains in its juice at least one per cent. of these undesirable compounds, while the beet-root juice under normal conditions has less than a tenth per cent. of them.

Then, again, there are other advantages on the side of the beet-root. The sugar of the sugar-cane exists in the stem of the plant, and is thus exposed to the risk of destruction by storms or other natural disasters; whereas in the beet-root the main sugar content is preserved underground in a safer situation. Again, if the sugar-cane is cut, it must be dealt with almost at once or much loss ensues, while the beet-root can be harvested and left in storage for months without needing to be treated. Finally, the sugar-cane is a tropical or sub-tropical plant, so that the extraction of sugar from it has to be conducted in places where skilled labour is hard to obtain, and where the conditions do not favour the use of fine machinery; whereas the beet can be cultivated in Europe with all the necessary skilled labour at hand. When these factors are borne in mind, the rise of the beet-sugar industry does not appear such a castle in the air as it seemed at first sight.

In the preparation of beet-sugar, the beets are finely ground and then steeped in water, which extracts the sugar-containing beet-juice and leaves various colloidal substances behind in the cells. To get rid of the acids which occur normally in plant saps, lime is added to the liquid, and this also has the advantage that it throws out of solution many albuminous materials which are not desired in the final product. The solution of sugar is then treated with certain harmless disinfectants which prevent fermentation setting in; and the liquid is evaporated, after filtration through animal charcoal, in vacuum pans. The crystals of cane-sugar which separate out are freed from the mother-liquor by treatment in a centrifuge, and are further purified to be ready for the market.

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Some idea of the importance which the beet-sugar industry has attained may be gained from the following figures. In the year 1907–8, the total sugar production of the world amounted, as far as we have statistics, to 13,847,000 tons; of this, 6,901,000 tons were derived from sugar-cane, while an even larger quantity, 6,946,000 tons, was beet-sugar. The production of beet sugar in Germany alone for that year amounted to 2,138,000 tons, or rather more than the output of India, 2,047,000 tons. Thus the sugar-cane is now taking a secondary place in the world's markets as a source of sugar.

This immense beet-sugar industry forms a striking example of the application of chemistry to the study of natural products. The original discovery of sugar in beet-root was made in 1747; half a century later the first beet-sugar factory was founded, and since that time the progress has been steadily maintained. But without the aid of chemistry, no one would have thought of the beet as a source of sugar, nor would the detection of its peculiar advantage—the lack of reducing sugars—have been made easy.

These are examples of one or two directions in which chemistry has touched upon the industrial field, and they might have been multiplied to a very great extent had there been space to enter into a discussion of them. The production of artificial flavourings and perfumes, the manufacture of artificial silk, the calico-printing industry, tanning, the soap-trade, to name only a few, are all closely connected with chemical processes. Enough has been said, however, to indicate the varied fields into which chemistry enters when it brings its methods to bear upon the manufactures of the world.

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CHAPTER IV

IMMUNO-CHEMISTRY AND SOME KINDRED PROBLEMS

IT is a curious fact that, while the practice of vaccination is almost universal in this country, very few people outside the medical profession take the trouble to find out the object aimed at in the operation to which they have submitted. The problem is of general interest, and in the present chapter an outline of the current views on the subject will be given, though, of course, it is obvious that many controversial points must be left untouched.

In a living animal, the blood plays one of the most important parts in the machinery of life. Not only does it act as a collecting agent for nourishment which, directly or indirectly, it transmits to the cells; but it is employed as a carrier for the various secretions which the cells produce; and, finally, it transports throughout the body the oxygen which is an essential factor in living tissue. Hence it is clear that in disease the blood must be an important factor, and that a knowledge of the blood constituents is essential.

An examination of a drop of blood under the microscope shows that it is made up of various types of cells (red and white corpuscles among others), and that these cells float in a yellowish liquid.

When some fresh blood (to which certain salts have been added to prevent coagulation) is placed in a glass

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vessel, it is found that after a short time it separates into two layers, the lower of which contains all the corpuscles mixed with a solid substance, while the upper layer is a clear liquid, the serum. Now if, instead of allowing the blood to stand, it be stirred with a glass rod while it is fresh, some matter will be found adhering to the rod; and when the latter is removed, the blood corpuscles no longer cake together on standing, as in the last case, but remain floating freely in the serum. This substance removed on the glass rod is called fibrin, and to it blood owes its property of coagulation.

Blood, therefore, is a mixture of at least three constituents which are easily separable from one another: the corpuscles, which are enclosed in walls of their own; the fibrin, which enables the blood to coagulate and seal up wounds; and, finally, the serum, this clear liquid in which the corpuscles float. It is mainly with the serum that we shall have to deal in the later portion of this chapter.

Let us now turn to another property of the blood. Certain animal extracts (rennet, for example) have the property of curdling milk. If, by means of a hypodermic syringe, some ferment such as this be injected into the blood of an animal, it might be expected, when only a very small quantity of this ferment is employed, that it would simply float about in the blood-stream without any particular effect; but experiment has shown that the actual result is much more curious. If small doses of the ferment be injected into the animal's blood at intervals over a period of time, the serum of the blood acquires new properties: it becomes capable of preventing the curdling of milk.

A numerical example will make the striking nature of this action apparent. Suppose that some of a rabbit's serum be added to fresh milk. No effect is noticed. The rabbit is next submitted to repeated injections of rennet ferment, and some of its serum is then added to fresh milk. Again

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there is no effect noticeable. This shows that neither normal rabbit serum nor serum after treatment with rennet has any curdling effect. Three specimens of milk are then taken from the same sample; to one of them two per cent. of the serum of an untreated rabbit is added, the second specimen is mixed with two per cent. of the serum of the rabbit after treatment, while the third specimen of milk is left pure. To these three liquids rennet ferment is added until curdling ensues in each case, and it is found that the results differ in the following way. Pure milk and the milk containing normal rabbit serum require only one part of ferment in three million parts of milk to ensure curdling, whereas the milk containing the serum of the treated rabbit needs a hundred and fifty times as much ferment (one part of ferment in twenty thousand parts of milk).

From this it follows that the serum of the rabbit, after repeated injections of the ferment, contains something which is not present to any extent in normal rabbit serum; and, further, that this new agent has an action directly neutralizing that of the ferment. The presence of the injected ferment has caused the animal's serum to produce a substance which counteracts the action of the ferment, and this new substance is formed in quantities far exceeding that of the injected ferment. This new product in the rabbit's blood is technically called an "anti-ferment" on account of its properties being opposed to those of the ferment itself.

Other cases are known in which an analogous "anti-compound" is formed. If a solution of snake venom be made, it can be diluted to any given strength which may be required. By making a sufficiently dilute solution, it is found that some of it can be injected into an animal without actually causing death; and by increasing this dose in the case of other animals of the same species it is possible to determine the minimum amount which it is necessary to

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inject in order to kill the creature. This quantity is called the minimum lethal dose. Suppose, now, that an animal has had a tenth of the lethal dose injected into its veins. It may suffer inconvenience, but it will not die. The treatment may be continued, a tenth of the lethal dose being injected at intervals, and at the end of ten injections the animal will obviously have had the minimum lethal dose passed into its system; but if the doses have been administered at sufficiently long intervals the tenth dose does not cause death. On the contrary, it is found that the animal is now able to absorb more than the original minimum lethal dose at one injection without suffering any great inconvenience, much less dying.

Experiments of this character show that by the steady injection of small doses the animal's system has become inured to the poison, so that a much greater dose is now required to produce symptoms which appeared after a small dose was given to the untreated animal. In other words, the animal has become immune to the effects of the poison, and its system has developed a resisting power far in advance of that which it originally possessed. A very rough parallel is provided by muscular development. Each of us is able to lift a certain maximum weight, which differs from person to person; but if we daily practise the lifting of smaller weights, we shall so increase the power of our muscles as to be able eventually to raise a weight much greater than our original lifting capacity permitted.

Let us return to the question of the ferment. It is clear that this ferment had some action on the rabbit's blood, and that in consequence of this action the blood produced an "anti-ferment," just as the injection of snake toxin produces an "anti-toxin" whose action neutralizes the effect of later injections of venom. Thus we are drawn to the conclusion that the blood has a general property of producing substances capable of counteracting the influence of deleterious

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compounds which are introduced into an animal's system, provided that time is given for the blood to produce these "anti-compounds" in sufficient quantity. If the full lethal dose be injected at a single operation, the blood has not sufficient time to improvise a defence against the poison; but if the lethal quantity be distributed over several injections at intervals, the blood is capable of coping with each dose as it comes, and, further, the resisting power of the blood is increased out of all proportion to the dose introduced.

A further point comes to light from a closer examination of these experiments. Suppose that a ferment be injected into a guinea-pig until the maximum ferment-resisting power is acquired by the guinea-pig's blood. It might be asked: "Has this treatment produced a general protective power, and is the guinea-pig now immune to snake venom?" Experiment shows that this is not the case; the guinea-pig will succumb to the minimum lethal dose of venom just as if it had not been treated with ferment injections. This proves that the formation of the resisting material is *selective*, and not general; the blood of the animal during the injections of ferment has been called upon to resist a particular form of attack, and it has produced a defence against that attack and that alone. In other words, each particular toxin which we inject into an animal's blood causes the production of the appropriate anti-toxin and no other.

Let us now sum up these results in a paragraph. There is a minimum lethal dose of toxin which, if administered *en bloc*, will cause death. If less than this dose be injected at intervals, the animal acquires a power of resistance against the drug, and will finally require much more than the original lethal dose to kill it. Further, each toxin in the blood stimulates the production of the appropriate remedy, and does not produce a general protective power;

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so that the *acquired immunity*, as it is called, is only an immunity against that particular toxin.

Consider in the next place another type of toxins, those which are produced by bacteria. When bacilli are kept in a solution which furnishes nourishment for them, they rapidly increase in numbers; and finally a fairly highly concentrated suspension of bacteria in the liquid can be obtained. Take the case of the diphtheria bacillus. If some of a suspension of these bacilli (which is called a "culture") be injected into a patient, the subject will develop diphtheria if he is susceptible to this disease. From this it might be deduced that the presence of the germs in his system was the cause of the ailment; but a further experiment would show that this is not the whole truth of the matter. It is possible, by using a sufficiently fine sieve, to filter off the germs from the liquid in which they float, and if some of this liquid, which contains no bacilli, be injected into a patient, he will develop diphtheria just as if actual germs had been introduced into his system. From this we see that the bacteria produce some toxins which find their way into the liquid in which the germs float, and endow it with the properties we originally ascribed to the bacilli themselves.

It remains to discover whether this behaviour is a general rule or merely a special case peculiar to the diphtheria bacillus; and in order to test this some other bacillus might be substituted for the diphtheria microbe in the experiment. If the bacillus which gives rise to typhoid fever were employed, it would be found that the results did not agree with those obtained in the case of diphtheria; for when the germs had been filtered off, the remaining liquid would not possess the power of causing typhoid fever when injected into a patient's system.

The explanation of these apparently incongruous results is easily found. In the case of the diphtheria bacillus, the

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toxins which this microbe secretes pass through the walls of its body and mix with the liquid of the culture; whereas, in the case of the typhoid bacillus, the toxins do not penetrate the skin of the bacillus, but remain cooped up within it. Thus in the culture of diphtheria germs the liquid becomes filled with free toxin, while in the typhoid culture the toxins are retained within the bacilli themselves. It can be shown that this is the case by grinding up the typhoid germs and thus breaking the cell-walls to such an extent that the toxins of the bacilli find their way into the liquid of the culture; if the remains of the germs be filtered off, it will be found that the liquid has now the property of producing symptoms of typhoid when it is injected into a patient.

Confirmation of the above line of thought is to be found by studying the course of the two diseases. In the case of diphtheria, the germs attach themselves to the surface of the body tissues and multiply superficially, causing the formation of a membrane. At the same time they excrete toxins which are absorbed into the system and give rise to other factors in the disease. On the other hand, an attack of typhoid fever may be divided into stages. In the first period there is the incursion of bacilli into the system; they enter the blood-stream and multiply there. At this stage no toxins can attack the system, as they are contained within the bacterial cells. Sooner or later, however, the bacteria die and their cell-walls disintegrate; with this disintegration the toxins are set free, and by their action the fever is produced. Thus as long as there are any bacteria dying in the blood-stream, we shall have fresh supplies of toxin injected into the system and prolonging the attack. From these considerations, one can see an explanation of diphtheria being a disease which rapidly reaches its climax, while typhoid fever is a much longer and slower process.

These examples will suffice to show that in the case of

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some diseases we are dealing, not with the action of bacilli themselves, but with the toxins which they secrete ; so that the question has been reduced to one which is more purely chemical in its nature.

The problem of coping with these toxins arises next. Since many cases of diphtheria and typhoid fever result in the recovery of the patient, it is evident that there are in the human system some factors which tend to neutralize the action of toxins. Let us see if we can frame an hypothesis which will enable us to explain this.

Assume, in the first place, that the body contains certain substances which we may call anti-toxins, whose character is such that they inhibit the action of toxins on the system. If these substances were present in sufficient quantity to neutralize all the toxins which an invasion of bacilli liberates in the system, then the subject would be immune from that particular disease. But in that case a second attack of the disease would find the body with fewer anti-toxins in it, some having been used up in neutralizing the first attack ; and the second attack would therefore probably be more severe than the first. This, as we know, is contrary to experience, for a second attack of scarlet fever, for example, is usually much less severe than the first attack.

We are therefore driven to make an addition to our hypothesis and to assume that the body is capable of manufacturing these anti-toxins from time to time, and that the entry of toxins into the system is met, not only by the anti-toxin present, but also by a further manufacture of anti-toxin. When all the toxins have been neutralized, if the patient recovers, he has present in his blood a large amount of anti-toxin in excess of that which he originally possessed, so that a renewed attack by fresh toxins is met by a greater quantity of anti-toxin than was the case in the first attack. Thus the second attack will be more rapidly coped with and will consequently entail less serious

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consequences than the first attack, which found the system comparatively unprepared.

The truth of this has been proved experimentally in a manner very similar to that employed in the cases of the ferment and the snake venom mentioned earlier in this chapter. The minimum lethal dose of a given infusion of bacilli is found for a certain set of animals; one animal is subjected to periodic injections of much less than the lethal dose, and it is found that this creature soon acquires a power of coping with doses far beyond the original lethal quantity. Suppose, for example, that 0·1 cubic centimetre of a certain culture is found sufficient to kill a rabbit. Less than this is insufficient to produce death. The rabbit is said to have a "natural immunity" corresponding to 0·09 cubic centimetre of the culture, for its system is normally able to cope with any dose up to this amount. Next, a similar rabbit is injected periodically with a dose less than 0·1 cubic centimetre of the culture. After a certain number of these injections the rabbit has been so strengthened against the toxin that it now takes an injection of, say, 0·3 cubic centimetre of culture to kill it. Its immunity, which formerly stood at 0·09 cubic centimetre, has been raised to 0·29 cubic centimetre; and this difference resulting from the treatment (0·29 cubic centimetre minus 0·09 cubic centimetre, *i.e.* 0·2 cubic centimetre) is the measure of what is called "acquired immunity." If the serum of this rabbit, which, on our hypothesis, contains a larger amount of anti-toxin than ordinary rabbit serum holds, be injected into an untreated rabbit, it is found that this second rabbit has a higher degree of immunity than the normal untreated rabbit. In other words, it in turn has been immunized, having received some anti-toxin along with the serum of the first rabbit. The first rabbit is said to have been "actively immunized," while the second rabbit has been "passively immunized."

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The foregoing paragraphs will have made clear the object aimed at in the process of vaccination. The bacterial injection which is given in the operation of vaccination is a very weak one; and what is aimed at is to produce in the body a corresponding anti-toxin which, in accordance with what has been said above, is formed in quantities far greater than are required for the immediate emergency. Thus after the vaccination has "taken," and the consequent indisposition has passed off, the patient's blood will contain a much greater amount of anti-toxin than was originally present before vaccination; so that when fresh germs of the disease make their way into the system, their toxins find awaiting them a quantity of anti-toxin sufficient to cope with them with comparative ease, and in this way the disease is warded off or reduced to a light attack.

So far, in this chapter, we have been dealing principally with actual experimental results; let us now turn to the current explanations of these phenomena. These may be divided into two classes, the chemical and the physiological. The former group of hypotheses take as a basis the conception that, when reduced to their simplest forms, the operations of immunity are purely chemical in their nature; they can be carried out in test-tubes as well as in the living subject, and therefore a real explanation of the immunity phenomena must be essentially chemical. On the other hand, Metchnikoff and his school refuse to accept this view as being a complete solution; for it fails to cover all the ground, though it answers very well within one field. The two lines of thought may be indicated quite simply. Blood, as we have seen, is made up of fibrin, serum, and corpuscles. The chemical school, of which Ehrlich is the most distinguished representative, take the view that the serum is the agent which plays the greatest part in producing immunity; whereas Metchnikoff and his followers maintain that the blood corpuscles act as defenders in the struggle; and that

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since these corpuscles are organized bodies it is impossible to accept a purely chemical explanation of the whole phenomena. It seems likely that the truth lies between these two contentions, and that both serum and corpuscles play a part in warding off disease. We may now examine the outlines of the two theories; but it should be clearly borne in mind that no attempt can be made, in the space at our disposal, to go into all the controverted points which have arisen.

On the purely chemical side, the explanation which makes the greatest appeal to us is Ehrlich's "Side-chain Theory," founded upon the following ideas. When the intimate structure of any organized creature is examined, it is found to contain various portions, such as a skin, a digestive apparatus, and so on. A closer examination reveals that each of these organs is built up of smaller aggregates which lead a semi-independent existence and which are termed cells. Each cell has to fulfil certain functions, without the continuance of which the whole machine would break down. In the first place, the cell has to obtain nutritive matter from outside; and, secondly, it has to incorporate this nutritive matter in its own system. Thus the exterior part of the cell, which comes in contact with foreign materials, has the function of collecting food which the inner part of the cell transforms into fresh protoplasm. How does the cell gather in its food-stuffs? According to Ehrlich, the exterior of every cell has attached to it a series of "side-chains" or "receptors" which act chemically, just as the arm of the octopus acts physically, in anchoring foreign bodies and bringing them into range of the digestive part of the creature. These "side-chains," however, have no selective power. They are not able to distinguish between useful and deleterious substances, but anchor to the cell nutritive bodies or toxins with equal readiness. Thus, if the side-chain hooks on to a

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nutritive substance, this is passed on to the central part of the cell and serves as food; but when a toxin is anchored, it chemically paralyzes the side-chain and prevents its further action. If there is a large influx of toxins into the blood, a cell may have so many of its side-chains paralyzed that it is unable, by means of the remainder, to secure adequate nutriment, and it consequently dies; and when a sufficient number of cells are thus killed, the whole system suffers a shock which may result in death.

Let us take a case where matters do not go so far, but where only a few toxins penetrate into the system. In these circumstances, a cell may have a few side-chains thrown out of action by toxins anchoring on them, and then, on Ehrlich's hypothesis, a peculiar phenomenon will result. It is a well-known fact that when vital tissue is injured, the repair of the injury is accompanied in many cases by a great over-production of the repairing material. For example, in the case of some slight injuries to the skin, callosities are formed, which consist of excess of skin tissue over and above what is required to repair the original damage. Suppose that the same thing occurs on a much smaller scale among the cells. A cell anchors a few toxins by means of its side-chains; these side-chains are thus thrown out of action, and the cell will, therefore, set to work to repair the damage by throwing up fresh side-chains to take the place of those which have been incapacitated. But just as in the case of the skin, the cell will produce *more* side-chains than are required to restore it to its original condition. The result will be that too many side-chains will be formed, and some of these, being of no value to the cell, will be broken off and thrown into the blood-stream.

Now comes a point on which the whole of Ehrlich's hypothesis turns. It has already been said that the original side-chains of the cell are not selective in their actions; they anchor anything which comes near them

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with a complete indifference to its quality. On the other hand, these new side-chains are formed with a particular object by the cell; they are intended to grapple with the particular type of toxin which is causing annoyance, and are therefore constructed in such a way as to grip a toxin in preference to any other body. Not only so, but they will only grasp the particular toxin whose presence brought them into existence. Of course, it will be understood that this is merely a metaphor, and is not to be taken as implying intelligence in the cell; chemical processes are all that are to be understood. The main point is that these new side-chains are *specific* in their action and not general, like the original side-chains. A rough simile might be taken from everyday things by saying that the toxin and nutritive matter are like two watches of different makes. Both of these can be wound up by an adjustable key which expands to fit any watch; this would correspond to the original side-chain which can grasp both toxin and food-stuff. On the other hand, the new side-chain would be represented by the rigid key of the one watch (toxin) which will not fit the other watch (food-stuff).

It is interesting to trace the subsequent effects of this casting loose of these specific side-chains. A side-chain becomes detached from its parent cell and drifts off in the blood-stream, but its breaking away from the cell has affected neither its specific character nor its power of hooking on to a toxin similar to that which produced it from the cell. It has become what is called an anti-toxin. If further toxins of this particular type enter the blood, this detached side-chain is just as capable of attaching itself to them as if it were fixed to a cell, and consequently the system has now at its disposal more side-chains than it had before, each of which is capable of fixing a toxin and neutralizing its effects. Thus, a second dose of toxin, instead of being wholly used up in paralyzing

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the side-chains of the cells, is in part anchored by the floating side-chains in the blood, and therefore less of it remains over to attack the side-chains of the cells. Hence we get the phenomena of immunity.

The diagram below may help to make the matter clear.

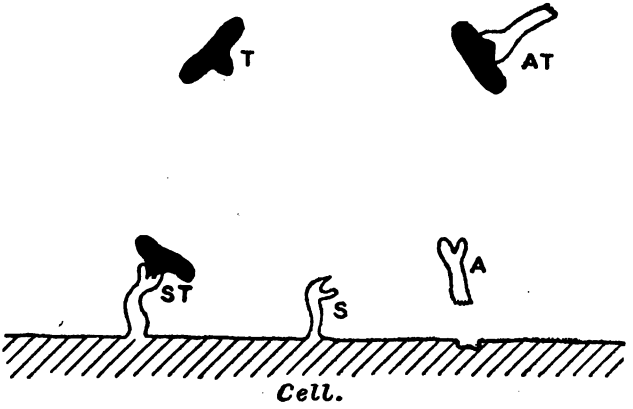

FIG. 2.

S = Side-chain or "receptor."
T = Toxin floating in blood-stream.
ST = Side-chain on cell with toxin anchored by it.
A = Free side-chain (anti-toxin) floating in blood.
AT = Anti-toxin with captured toxin.

So far we have been dealing with the noxious actions of the toxins which the bacteria secrete, but further consideration will show that this is only a section of the main problem of immunity. Take as an example typhoid fever, which we have already mentioned. It will be remembered that the typhoid bacteria secrete toxins which are injected into the patient's system only on the death of the germ. Now, if the germs were able to multiply freely in the system, a continual series of them would be dying in turn in the blood, and as they increase in numbers very rapidly in ordinary blood, the system would receive increasing doses of toxin,

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which would be very hard to counteract. Two questions at once suggest themselves. First, Do the typhoid bacteria multiply in the blood of a typhoid patient with the same rapidity as they do in a culture? and, second, Is there any machinery in the blood to deal with the bacteria in any way analogous to the defence against toxins?

Let us see what has been discovered experimentally with regard to these points. When a drop of normal serum is placed under a microscope and some typhoid bacteria are added to the serum, the bacteria move about rapidly and multiply fairly quickly. To this normal serum a very dilute solution of the serum of a typhoid patient is added, and at once a great change takes place. The bacilli seem to lose their power of rapid motion, and they clump together into large aggregates. This phenomenon is so marked that it can be observed even with the naked eye when a sufficient quantity of the materials is used. In this form it is called Widal's reaction, and is employed in the diagnosis of typhoid fever.

There must be some explanation for this peculiar crowding together of the bacilli when the serum of a typhoid patient is present, and the simplest solution which presents itself is that under the influence of the typhoid serum the bacilli exude some sticky substance which causes them to adhere readily to surrounding objects. This hypothetical sticky substance we may call an "agglutinum," and the substance in the typhoid serum which causes its exudation from the bacteria may be termed an "agglutinin." It will be noticed that the process of agglutination does not kill the bacteria; it merely renders them less motile and less able to multiply than they were before.

Taking his stand on these ideas, Ehrlich has applied his side-chain theory to this case also. He assumes that some of the side-chains of the body-cells are complex and are built up of two groups: a haptophore group for seizing the

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corresponding side-chain of the bacillus, and a toxophore group, which influences the bacillus in some way and causes it to exude the agglutinating substance. This last may be supposed to be a compound which shields the bacillus from injury by the toxophore group. Unless the bacillus is gripped by the haptophore group of the cell, the cell's toxophore group has no chance to attack the bacillus. A rough analogy can be got by likening the cell to a scorpion which was forced to grip its prey with its jaws (haptophore group), before it could attack it with the sting in its tail (toxophore group). Now, when a bacillus approaches a cell, many of the cell's haptophore groups are used up in seizing the corresponding side-chains of the bacterium. The result here is the same as in the case of ordinary side-chains: the cell is stimulated to an over-production of fresh groups, and some of these new ones are thrown off into the blood-stream. There they meet with other bacilli to which they attach themselves, and by the action of their toxophore groups they force the bacilli in self-defence to exude the sticky agglutinum, thus causing the germs to clump together as we have seen they do in actual practice. The diagram on the next page gives a representation of the process involved.

In the last few paragraphs we have seen Ehrlich's conception of two methods by means of which bacilli may be hindered in their noxious action. In the first place, there are the anti-toxins, which neutralize the toxins exuded by bacteria either during life or after death; and, in the second place, there are the agglutinins, which act by making it more difficult for the bacteria to move about or breed. But these defences leave the main object of the animal body unfulfilled, for it is not sufficient to render toxins harmless or to prevent an increase of bacteria in the system. In order to end the disease there must be some means of actually killing the bacteria themselves and so getting to

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the root of the matter. Let us see how Ehrlich supposes this to be accomplished.

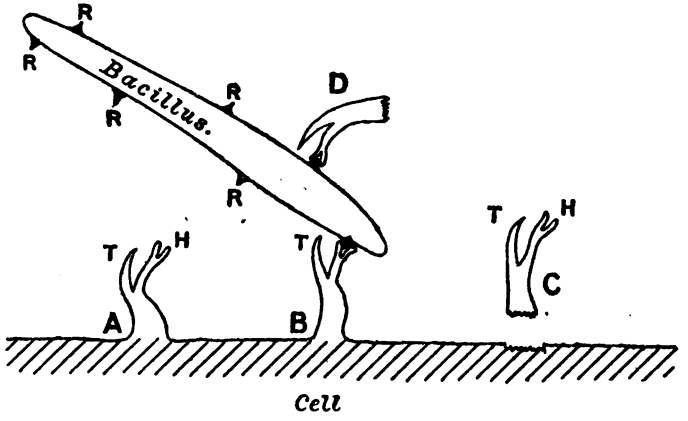

FIG. 3.

A, Side-chain with haptophore group H and toxophore group T.

B, Side-chain showing bacillus anchored by haptophore group seizing receptor of bacillus while toxophore group of side-chain attacks bacillus.

C, Side-chain cast off by cell and floating in the blood-stream, constituting an agglutinin.

D, Agglutinin anchored to bacterium by latter's receptor and acting with toxophore group on the bacterium.

H, Haptophore groups for anchoring bacteria.

T, Toxophore groups capable of attacking bacteria when anchored by haptophore groups.

R, Receptors of bacterium.

The size of the agglutinins in the above diagram is of course greatly exaggerated in comparison with the bacterium.

If bacteria be placed in normal blood-serum, a good many escape destruction, but some at least are killed. Normal serum therefore contains a little of some substance which acts as a bactericide. Now, it is found that if a person be immunized against, say, typhoid fever, his serum gains in bactericidal power and is able to kill more

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microbes than normal serum can. Suppose that we estimate how many bacteria a given quantity of this immunized serum will kill. It is much more active than normal serum. We now heat it to about 60° C. and test it again with bacteria, and we find that it is no longer fatal to typhoid bacilli. What has happened? Obviously, by our heating of the serum we have destroyed some component of the serum which is essential for the killing of the bacteria, and which breaks up at a temperature of 60° C. The next question which arises is: Does this substance exist in ordinary serum or only in typhoid serum? We can prove that it does exist in normal serum in a very simple way. We take the immunized serum, whose power we have destroyed by heating and which will kill only a few bacilli, and we mix it with some normal serum which also has only a feeble bactericidal power. On testing the mixture, we find that it is as active against bacteria as the original immune serum was. Thus the power which was lost by heating the immune serum is regained by the addition of a trace of normal serum.

These facts, which at first appear strange, can be easily accounted for. Suppose that in order to kill a bacillus two separate substances must be present. One of these is found in normal serum, and is so sensitive a compound that it is destroyed even at the low temperature of 60° C. The second factor in the case is found only in immunized serum, and is more resistant to the action of heat, so that it is unaffected by the temperature being raised to 60° C. In addition to this body, immune serum contains the same substance as ordinary serum. Thus in immunized serum both factors are present, and the serum is able to destroy bacilli; but when the serum is heated to 60° C. one of the necessary compounds is broken up, with the result that the serum is no longer able to attack the bacilli. But this unstable compound is present in ordinary serum, so that by

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adding some ordinary serum to the immunized and heated serum we have again both the necessary factors present in the mixture, with the result that the mixed sera are able to attack bacilli with the same vigour as immunized serum.

Let us see if Ehrlich's ideas will help us to get a clearer conception of the process. According to him, bacteria in themselves do not injure the cells of the body directly; their agent of attack is the toxin which they secrete. But bacteria have on their surface a series of side-chains (receptors), by means of which they can be seized by the side-chains of a cell in search of food; and when a bacterium is thus anchored to the cell, it throws out of action the side-chains of the cell which are used up in attacking it, and consequently the nutrition of the cell suffers owing to the lack of free side-chains. To cope with this emergency, the cell has a series of special side-chains which exist for a particular purpose. They contain two groups—a haptophore (or seizing group), and a receptor (or seizable group). The latter group is supposed to become active only after the haptophore group has gripped an external substance. Let us imagine that a bacterium approaches the cell. It is seized by the haptophore group and anchored to the cell in the usual manner. As soon as this has taken place, the latent receptor group becomes active and capable of grasping a substance normally present in the blood. This last body is termed a "complement." Neither receptor nor complement alone is capable of attacking a bacillus, but in conjunction they are able to kill it. An analogy may be found in the case of a revolver and its cartridges; neither revolver nor cartridge by itself is capable of killing a man at thirty yards range, but together they are sufficient. As soon, therefore, as the complement attaches itself to the cell's receptor side-chain, the bacillus can be destroyed.

But here again it must be assumed that these special side-chains are thrown out of action in anchoring the

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bacillus, and that an over-production of similar side-chains is begun by the cell to counteract this. Some of these side-chains break away and drift into the blood. In this state they are called "intermediate bodies," or "amboceptors." If one of these floating side-chains meets a bacillus with the proper type of receptor, it attaches itself to the bacterium by means of the haptophore group; and then the latent receptor group of the intermediate body secures one of the complements which are floating in the blood. Then there are now anchored to the bacillus the two factors necessary to its destruction, the side-chain and the complement, and the bacillus is killed.

To explain the effects of heat, it is only necessary to assume that the complements which are found in normal blood can be destroyed by raising the temperature to 60° C. The intermediate bodies (floating side-chains) will still be left, and these may anchor themselves to bacilli; but they can do the latter no harm until a supply of fresh complements is provided. This is done by adding some normal blood, which contains complements but no intermediate bodies.

To make this rather complex statement clear the following table may be of some help :—

	Contains	
Normal serum	Complements.	No intermediate bodies present; therefore serum does not contain necessary pair of agents for killing bacilli.
Immunized serum	Complements and intermediate bodies.	Both agents present. Bacilli can be killed.
Immunized serum after heating	Complements destroyed. Only intermediate bodies left.	Only one agent present. No action on bacilli.
Immunized serum after heating, plus Normal serum	Intermediate bodies. Complements.	Both agents present. Bacilli can be killed.

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The action of the complement and the intermediate body is depicted in the diagram below :—

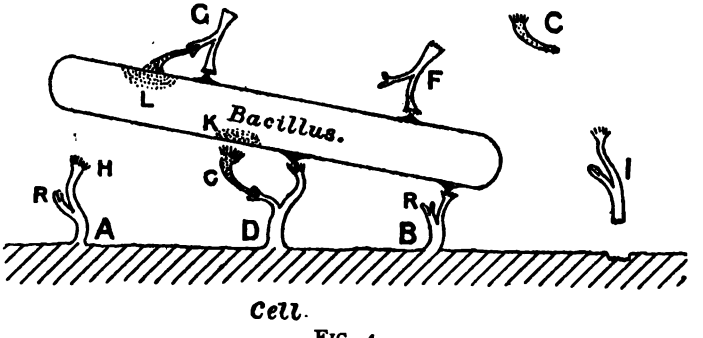

FIG. 4.

A, Side-chain with active haptophore group H and inactive receptor R.
B, Side-chain anchoring bacillus by haptophore group; receptor R now active and waiting for complement.
C, Complements.
D, Side-chain with complement C attacking bacillus at K.
I, Intermediate body (detached side-chain floating in blood).
F, Intermediate body anchored on bacillus, receptor active, waiting for complement.
G, Intermediate body and complement destroying bacillus at L.

The sizes of complements and intermediate bodies in the above diagram are greatly exaggerated in comparison to the bacillus.

There are two points which must be noted in connection with the above. In the first place, the intermediate bodies —side-chains thrown off by the cell and furnished with haptophore and latent receptor groups — are specific in their character; that is to say, they will attack only the same kind of bacillus as the one they were produced to cope with. An intermediate body produced in response to an invasion of typhoid bacilli cannot attach itself to a diphtheria bacillus. On the other hand, the complements are general in their character; they can be seized upon by any type of intermediate body and used by it against any bacillus which the intermediate body can fix itself upon.

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We can now take a general view of the machinery which, on the chemical theory of immunity, the body employs to ward off bacterial attack. In the first place, since the toxins are the real source of danger, the system produces a series of anti-toxins to cope with these directly. Then, since a rapid multiplication of bacteria in the blood will be followed by an increase in the amount of toxin exuded by them, the body produces the agglutinins which serve to hinder the movements and decrease the breeding power of the bacilli. Finally, since safety can be attained only by the elimination of the bacilli, the intermediate bodies are thrown into the blood-stream, and complements are provided which can destroy the microbes when anchored to them by the intermediate bodies.

At first sight it seems that the body has furnished itself with more means of defence than are necessary, for if the anti-toxin and the complements (with the appropriate intermediate body) were present in sufficient numbers, both toxins and bacteria would rapidly be rendered *hors de combat*, and the reason for the employment of the agglutinins appears obscure. A little consideration shows that this is not so. Take the case of typhoid fever as an example. Here the destruction of the bacilli by the complements in the blood is followed by the freeing of a large amount of toxin normally contained within the bacillus. Hence, if the blood contained a large amount of complements, very many bacilli would be killed off simultaneously, and a great quantity of toxin would be launched into the blood at once, which would throw a considerable strain upon the anti-toxin branch of the defence. The agglutinins are supposed to play their part at this point, by preventing a rapid multiplication of bacilli; so that it becomes possible to kill off the latter in small batches, and thus minimize the amount of toxin which finds its way into the blood-stream at any given moment.

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The foregoing paragraphs give the outlines of the most comprehensive chemical theory of immunity, but this is not the only view which is in the field. Arrhenius, Madsen, and others have endeavoured to explain immunity on the assumption that the chemical reaction between toxin and anti-toxin is very similar to that which takes place when a weak acid is neutralized by a weak base. This suggestion, however, seems to be open to serious objections from a physico-chemical point of view. Bordet has proposed the idea that immunity phenomena may properly be compared to the state of things observed in adsorption, and this suggestion brings immunity into touch with the chemistry of colloids and coagulation; but the discussion of his views would lead us too far.

We must now deal with the views of Metchnikoff and his school, but as these are of a physiological rather than a chemical nature, a detailed treatment of them hardly lies within the scope of this volume. For the sake of completeness, however, their main lines must be indicated.

It will be noticed that in the Ehrlich theory, the whole process of immunity depends upon the serum of the blood, and that no account is taken of the action of the blood corpuscles. Metchnikoff has shown, however, that these corpuscles do actually play a very important part in the struggle against disease, for some of them, the phagocytes, have the power of enveloping and digesting bacilli, which are thus destroyed. It might be thought that even if this were the case their influence would be small on account of their apparently haphazard movements in the blood-stream, but it has been conclusively proved that these are by no means random actions; on the contrary, the phagocytes have the power of seeking out the foreign bodies which they destroy. By introducing under the skin of a frog a tiny capillary tube containing a few bacilli or a trace of some chemical, it is possible to study the movements of the

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phagocytes in the vicinity, and it is found that they are either attracted to or repulsed from the capillary tube, according to the nature of the substance it contains. This apparent sensibility of the phagocytes is termed chemiotaxis. If the capillary tube be filled with dead microbes, it is found that phagocytes are attracted to its neighbourhood; this is called positive chemiotaxis. Whereas, if the tube contains lactic acid or glycerine, the phagocytes retreat from it; and this is termed negative chemiotaxis. Sir Almroth Wright has suggested that in this way the serum comes into action, coating the bacilli with some substance which induces positive chemiotaxis, and thus attracting the phagocytes and inducing them to devour the bacilli. This substance he terms an opsonin.

It seems probable that, when the whole truth of the matter is known, it will be found that neither the chemical nor the physiological view alone will suffice to account for the phenomena of immunity. The chemical side of the question, taken alone, leaves out of consideration the activity of the phagocytes; while the upholders of the physiological hypothesis must finally come back to chemical explanations in order to account for chemiotaxis and the actual digestion of the microbe by the liquids within the corpuscles.

An account of the phenomena of immunity would be incomplete without some reference to the peculiar process which has received the name of anaphylaxis. It was observed by the discoverer of this curious phenomenon that if a poison obtained from sea-anemones was injected into a dog, a small dose produced very little effect, while a heavy dose led to most serious results. On carrying the investigation a stage further, he injected a trifling dose into a dog, and then, following the usual course in the attempt to immunize the animal, injected a second small dose about three weeks later. To his astonishment, the second dose produced most serious symptoms in the dog, just as if it

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had received a large over-dose. It is evident that the first small dose had produced in the dog a state of hypersensitiveness to the poison, so that on the injection of the second small dose a serious break-down of the animal's system occurred. This condition of hypersensibility is termed anaphylaxis.

Later work showed that poisons were not the only substances which could produce the anaphylactic condition. Horse serum, which is a comparatively harmless substance, can call anaphylaxis into existence; and even albumen, such as white of egg, appears to be sufficient. In some experiments with guinea-pigs, it was shown that extraordinarily small quantities of substances injected were able to produce the hypersensitive condition; thus in one case even a trace of white of egg amounting to 0·000,000,05 grm. (or less than 0·000,000,002 oz.) was found to be enough. Further, in the case of the guinea-pig, the hypersensitive state appears to be transmitted from the mother to her progeny.

At the present time, the explanation of this peculiar phenomenon is not known, but it is generally assumed that anaphylaxis represents the first stage in the immunity process, and is due to the over-production of some substance which in excess has an ill-effect upon the organism, but which only becomes active in presence of a further injection of the original toxic substance. The study of the subject is still in its early stages, the first experiments having been made in 1905, so that at present there is hardly enough experimental material at hand to base theories upon. There seems, however, to be no doubt that with the increase of our knowledge in this field a much deeper insight into the immunity problem as a whole will be gained.

In concluding this chapter, attention must be directed to one or two applications of the phenomena which we have already described, for our knowledge of the chemistry of the

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blood and its action has recently been applied to the solution of problems which at first sight appear entirely removed from its field.

Suppose that we inject some human serum into a rabbit in small doses at intervals. After the rabbit has undergone this treatment for some time, it is observed that the serum of its blood has acquired a new property. On mixing the serum of the immunized rabbit with some serum from human blood, a cloudy appearance can be seen, due to the presence of a precipitate. On repeating the second part of the experiment, but using horse serum instead of the human serum, no precipitate is produced. This appears at first sight merely to be a point of purely scientific interest; but let us now turn to a particular application of it, when it will be seen to be of immense importance.

Take the case of a man accused of murder. The police find that some of his clothes are stained, apparently with blood. The accused states that he is a butcher, and that the stains are probably due to the blood of a cow or a sheep. How can we settle definitely whether the blood in question is human or not? A rabbit is injected with human blood until it acquires immunity. Its serum is then able to produce a precipitate with human serum, but will show no similar precipitate with the serum of other animals. All that we need do is to wash off some of the blood-stains with salt water, filter off any solid material, and then apply the precipitation reaction. If the blood on the clothes is human blood, the precipitate will prove it; if it is merely sheep blood, there will be no precipitate, and a very strong confirmation of the accused's statement has been obtained.

Next, take a more purely scientific application. It is found that if a rabbit be injected with the blood of a dog the rabbit serum will form a precipitate with the serum of a dog's blood; but, further, it will precipitate also the serum of a fox, which is closely allied to the dog in the biological

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scale. Again, a rabbit immunized with horse's blood will give a precipitate with the sera of donkeys and tapirs, both of which are closely related to horses. It is evident that this reaction has put into the hand of the biologist a weapon by means of which he can discover the closeness of relationship between various animals; those which are nearly related giving the same response to the test, while those which are in no way connected biologically will give different results. The most interesting result hitherto obtained has been the establishment of the fact that human blood and the blood of the anthropoid apes give a similar reaction, while the blood of some monkeys shows no affinity with the blood of man.

It will now be evident to the reader that the processes involved in serum therapy, as it is called, are complex in the extreme; and it will also be clear that the method, though effective, is a somewhat circuitous and wasteful one. The main objectives before the physician are the destruction of the microbe and the counteracting of the toxins which it produces; but the methods of serum therapy entail upon the organism a considerable amount of work which might conceivably be avoided by more direct attacks. We have seen that the serum therapist, by his injections, forces the organism to synthesize the reagents necessary to cope with bacteria and toxins; and it must occur to every one that this roundabout process might be avoided if we could inject some drug which would attack the microbe directly without the intermediation of naturally synthesized products. The pioneer in this new line, to which the name chemotherapy has been given, has been Ehrlich. Let us see the nature of the problem before him.

If a dose of a drug be injected into an animal infected with any disease, it is clear that we have various factors to take into account; for the drug may affect either the microbe or certain cells of the organism, or it may exert a deleterious influence upon both microbe and organism; and

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since the drugs we employ must be powerful in order to destroy the microbe, we run a great risk of damaging the organism at the same time. To avoid this, we must seek for a *selective* drug which will attack the bacilli, but will have the least possible influence upon the cells of the organism.

At first sight, the problem appears to be so complicated as to defy solution, but a way out of the difficulty was soon discovered. It was well known to chemists that certain dyes would give a fast colour with, say, silk ; while the same chemicals when applied to cotton did not fix themselves in the fabric. Now, the operation of dyes of this type depends upon a chemical reaction taking place between the dye and the fabric to which it is applied: when there is no interaction between the two, the dye simply lies mechanically in the fabric and can be washed out again ; but when a chemical compound of fabric and dye is formed, then a fast dye results.

Turn now to the case of bacilli. It is found that these organisms can be stained with certain dyes, but not with others ; so we may assume that when a bacillus is stained, a chemical reaction takes place between its material and that of the dye, whereas when no staining occurs this reaction is absent. Further, certain dyes will stain bacilli, but will not stain the tissues of the human organism.

Now, if a given dye is of this type, staining a bacillus but not affecting the human tissues, it is clear that it is a selective agent. It can fasten itself upon the bacillus, but will not interfere with the normal action of the human organism. The discovery of such dyes, then, gave us the weapon which was required.

But the dye in itself is of little value, for even a stained bacillus might live quite comfortably and produce toxins as well as its unstained neighbour ; a more deadly agent must be employed against it. Let us take a specific case, that of syphilis. This disease is produced by a bacillus known as

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spirochætes pallida. Now, a study of this microbe has shown that it is peculiarly sensitive to mercury; in fact, mercuric chloride (corrosive sublimate) kills it instantly. This action was known empirically long before the bacillus itself had been detected. Mercury compounds, then, might be regarded as desirable agents to inject into an organism in order to exterminate the spirochætes; but unfortunately mercury has a very deleterious influence upon the human organism.

At this point we might have recourse to the dye of which we have already spoken. Suppose that we could introduce mercury into combination with the dye and then allow the dye-mercury compound to act upon the spirochætes; it seems probable that, since the dye does not affect tissue, the dye-mercury complex would confine its action to the bacilli and leave the human tissues untouched. In this way the destructive action of the mercury would expend itself upon bacilli and the latter would succumb.

Unfortunately, it is found that these dye-mercury complexes are unstable under the conditions of treatment; the mercury is not retained by the dye, and consequently it is set free to attack the tissues of the patient. Thus, the idea of using mercury as a bactericide in this way has been abandoned.

The next problem before investigators was to discover some element which could be used instead of mercury, and at the present time arsenic seems to be the most successful substitute. But this research has cost an enormous labour, so great that only experts can realize its full magnitude. The drug which was aimed at was to contain two constituent parts: first, a group which would take the part of the dye we have described, and which would fasten itself upon the spirochætes without having any effect upon normal tissue; and, second, a toxic group containing arsenic or some such element, which would damage the bacillus when brought into combination with it. In other words, the drug Ehrlich

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was trying to produce might be likened to a kind of poisoned arrow with a barb and a poison bulb which would come into play when the barb had gone home in the bacillus, or a sort of chemical scorpion with the dye as its claw and the arsenic atom as its sting.

Now, at the time when Ehrlich began his researches in this line, very few organic compounds of arsenic were known, so that his work really comprised three lines of inquiry: first, he had to devise methods of preparing many unknown derivatives of arsenic; secondly, he had to synthesize numerous kindred substances—for a very slight variation in constitution is found to have great influence upon the action of these substances; and finally, he had to conduct a long series of tests of the relative values of his new synthetic compounds. Some idea of the magnitude of these investigations may be gathered from the fact that real success was only obtained with the six hundred and sixth compound made—Preparation No. 606, or salvarsan —and further advances have resulted in the production of over eight hundred of these new bodies.

The results obtained with 606 have been of very great importance; and now that the line of research has been opened up it is probable that even greater advances are to be expected. In the treatment of syphilis and that of sleeping sickness, chemotherapy has shown that it can achieve brilliant results; and as time goes on other bacterial diseases will be drawn within its scope; so that it seems probable that its coming will be a landmark in the history of the long combat with disease.

In the foregoing pages, a rapid survey of the field of immuno-chemistry has been given, though naturally many points of interest have been omitted, as their consideration would have led us too far from the main track. It must be borne in mind that, to the scientific intellect, theories are merely tools and not masters. A theory is merely an

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instrument by means of which the scientific man classifies his experimental data, and forecasts further possible lines of research. Investigations along one of these new lines may lead to discoveries which prove the current theory to be untenable; it has then accomplished its purpose, and is thrown aside to make way for something better. In course of time it is not improbable that Ehrlich's side-chain theory may share the fate of many others and be discarded in favour of some newer and more comprehensive view; but by that date it will have amply justified itself in the enormous outpouring of experimental investigation to which it has given rise. Intricate as it is, it gives some idea of the complexity of the interactions which go on from day to day within the human blood.

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CHAPTER V

COLLOIDS AND THE ULTRA-MICROSCOPE

AMONG the most curious features in the progress of science are those cases in which a subject proceeds up to a certain point and then lies dormant for many years until new methods have been developed which bring it again into prominence. A striking example of this intermittent advance is furnished by the chemistry of colloids.

The investigations of Graham in the sixties of last century opened up the field; but apart from his researches, very little interest was taken in the subject for nearly fifty years. Then, with the rise of physical chemistry, came a revival, and in the opening years of the present century researches on all lines of colloid properties were carried out, which soon increased to such a flood that a new scientific journal had to be founded in which they could be collected. The subject has been found to touch so many other fields in chemistry and industry that it seems destined to take its place as one of the subdivisions of chemistry and to extend its ramifications into biology and physiology.

The fundamental researches of Graham can be very briefly described. If an animal membrane, such as a pig's bladder, be stretched across the end of a cylinder in such a way as to form a drum-head, we have an instrument which is termed a dialyzer. When this membrane-covered end is immersed in water, the liquid cannot rush into the cylinder all at once, but it is found that it slowly permeates the membrane and oozes into the drum. If a solution of

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common salt be substituted for the pure water used in the first experiment, it is found that salt water penetrates into the drum. Thus the animal membrane does not bar the passage of the salt through it. We now ask ourselves, Would any other dissolved substance pass with the water through the membrane into the drum? Graham tried various bodies, and found that they appeared to do so; but when a solution of glue was employed, he found that it had not the property of passing through the membranous screen. Starch and some other substances also failed to penetrate. Thus it soon became clear that the dialyzer furnishes a method of separating substances into two classes: those which would pass through the membrane and those which did not appear to have this power. A more careful examination of the representatives of the two classes showed that they differed from each other in a most important physical property. Substances which passed through the membrane readily were found to exist in the crystalline form (like common salt, baking powder, sugar, and so forth); while the other substances, which showed less capability of penetrating into the interior of the dialyzer, were found to be amorphous, non-crystalline bodies like glue, starch, and gelatine.

Thus it became customary to speak of *crystalloids* and *colloids* (from κόλλα = glue), the members of the one class being crystallizable bodies, while the other contained amorphous compounds which could not be obtained in crystalline form.

Investigations were next undertaken upon the colloid class, and it was soon found that this in turn could be divided into two groups. White of egg is a colloid. In its ordinary state it can be dissolved in water, with which it forms a clear solution. But if we boil this solution, we find that the white of egg changes its character and comes out of the solution in the form of a white solid which is insoluble

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in water. The solution of white of egg in water is called a colloid solution or *sol*, while the insoluble white of egg is termed a *gel*. Another example will make the matter clear. Let us take a hot, strong solution of gelatine in water. When it is heated, it forms a liquid and is said to be in the sol form, but on cooling it solidifies to a jelly-like mass which is termed a gel. The process of converting a sol into a gel is known as coagulation. From these data, it is clear that colloids may be divided into two sections: the colloid sols or colloidal solutions, and the colloid gels.

At first sight it may appear strange that phenomena of this type should be dealt with in the field of chemistry, as they seem to be physical rather than chemical in their nature. In practice, however, the subject has been handled by the chemist more than by the physicist for two reasons. In the first place, the preparation of these substances belongs to the chemical field, and most of the recent research has been led up to through chemical investigations. Secondly, the subject of colloids plays a great part in physiological problems; and at the present day organic chemistry is coming more and more closely into touch with physiology: so that in this way the importance of colloids has been brought home to both organic and physiological chemists. Most of the more complicated animal and vegetable products are colloidal in nature; and as these are of the greatest interest from the chemical standpoint, it is natural that colloidal properties should excite the interest of chemical investigators.

Various chemical processes result in the formation of sols, but with some of these we cannot deal in this place, as they are too technical in character. There are, however, two methods which are easily comprehensible. It has already been pointed out that a strong solution of ordinary gelatine in hot water is a sol, and that by cooling it the corresponding gel can be produced. The reverse

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process may be carried out by warming the solution again, when the solid jelly liquefies and is converted once more into the sol form. The same result can, in some cases, be achieved without the intervention of heat. A substance known as silicic acid has been shown to exist either as the gelatinous gel or as a liquid sol. Graham found that the addition of a mere trace of a salt will convert the gel form into the sol state. In such cases as these, however, either the sol or the gel is used as a starting material, and the process does not seem to throw much light upon the colloidal condition.

The second method of preparing colloids suggests more. Bredig found that when an electric arc was allowed to play between two metal rods under water, small particles of the metal were wrenched off from the electrodes and floated about in the water. After filtering the water through blotting-paper, he noticed that the liquid which passed through, though it contained no particles which could be seen even with a powerful microscope, still bore with it some portion of the metal, and he was able to prove that in certain respects this liquid behaved exactly like an ordinary colloidal solution. This suggests that colloidal solutions contain a series of very fine particles suspended in the liquid; for we cannot imagine that metallic gold, for example, is truly dissolved in the water in the case of a gold sol. The problem before us is to discover some means by which these particles can be seen and some estimate of their size and numbers can be formed.

The first suggestion which occurs to the mind is to turn a microscope upon the solution, and by its aid observe the nature and number of the particles; but when this is tried even with the most powerful instruments, no floating masses are detected in the liquid. Obviously, then, if the colloidal solution contains particles, they must be of an extremely small size.

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At first sight, then, the microscope has failed us; but further inquiry shows that this is not the case. What is required is a special arrangement of the microscope. The problem was solved in an exceedingly simple manner by utilizing a phenomenon with which every one is familiar. Suppose that we go into a well-lighted room. We know that the air is full of small floating dust-particles, but we do not see them owing to their extreme minuteness. But if we darken the room and then admit a fine beam of light into it through a hole in a shutter, we see in the path of the beam a host of dancing motes, which are really particles of dust. This shows that if we illuminate a series of floating particles by a beam of light while the rest of our field of vision remains in comparative darkness, we are able to detect particles which under ordinary conditions would be so small as to escape our observation.

Now apply this to the case of the particles in the colloidal solution. So long as the whole field of the microscope is illuminated we shall fail to detect the particles; but if matters are so arranged that a beam of light is thrown upon the particles while the eye is shielded from the direct rays of the light-source, we shall be producing a state of affairs parallel to the case of the motes in the beam in the darkened room; and in this way we may be able to see each particle as a bright speck on the dark ground of the field.

The instrument which was devised on these lines is termed the ultra-microscope, on account of its being able to render visible numerous particles which were too small to be detected by an ordinary microscopic examination. The arrangement employed is quite simple. An ordinary microscope is fitted up for observation in the usual manner. The field of the instrument is shaded from rays of light falling upon it, so that on looking through the eyepiece a dark field is seen in which nothing is visible. A brilliant

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ray of light is now thrown on the solution to be examined, in such a way that the beam is at right angles to the line of vision of the observer through the microscope. Then, if there be any small solid particles in the path of the beam, they are brilliantly lit up, while the rest of the field remains in darkness; and thus it becomes possible to get an idea of the manner of particles present in a given portion of the solution under examination.

The extraordinary sensitiveness of this apparatus may be best gauged from the fact that it has been shown that by its aid we can detect particles whose diameter is as small as one five-millionth of an inch (0·0000002 inch).

So much for the first part of the problem. Let us now see how the second half of the question has been answered. How can we form an estimate of the number of such extremely minute bodies in a given volume of solution? At first sight, the counting of these particles in an ordinary solution might well seem an impossible task. Not only are they minute, but they are not fixed in their places, and consequently it might appear beyond our powers to avoid reckoning the same particle twice or three times over as it moved from point to point in the field, while the number of particles in an ordinary colloidal solution is so vast that even if other difficulties were removed the time occupied in mere counting would be extremely long.

The task is not, however, nearly so laborious as it might appear to be, though it does involve a good deal of trouble. Suppose that a colloidal solution contains a very large number of floating particles. When it is placed under the ultra-microscope, the field appears to be filled with a luminous haze, and it is very difficult to distinguish one particle from another. A given volume of this liquid is now diluted, say, a hundred times, and by this means the floating particles are spread out through a greater volume. Let us suppose that we start with a cubic inch of the solution and dilute it

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with water to one hundred cubic inches. It is clear that we still have the same total number of particles in this new solution as were present in the original cubic inch, but we can separate off one-hundredth part of this number from the rest by withdrawing a cubic inch of the new solution by means of a pipette. But one cubic inch of solution is far too large a quantity to place on the slide of the ultra-microscope, so we may take one-hundredth of a cubic inch. This will contain $\frac{1}{10000}$ of the original particles. Suppose, on looking through the ultra-microscope, that thirty particles are visible in the field. Obviously these thirty particles are $\frac{1}{10000}$ of the total number of particles present in the original cubic inch of solution; so we see at once that the original cubic inch of solution must have contained 30 × 10,000 or 300,000 particles. Thus the whole work consists in taking a fixed quantity of solution, diluting it to a known volume, withdrawing a certain quantity of this new solution and counting the few particles in it. Then, as we know the fraction of the original solution which our specimen represents, we have only to multiply the corresponding figure by the number of particles found in the specimen and thus get the required result. Of course the method is only a rough one, but it is usual to carry out a series of experiments and take the average, by which process the error is considerably reduced.

The ultra-microscope has been applied to the study of another set of phenomena which, in one aspect at least, are fairly familiar. When several examples of stained glass were examined, it was found that the ultra-microscope revealed the presence in the glass of extremely minute particles, which were embedded in the transparent glass, and which were so fine that no ordinary microscope could reveal their existence. Thus apparently in many cases the tints in stained glass are due, not to a colouring matter evenly distributed through the vitreous material, but to a series

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of extremely finely divided particles of metal embedded in the glass.

At this point it may be asked, In what way do colloidal solutions differ, on the one hand, from ordinary solutions, and, on the other, from suspensions of solid matter in water, such as we can get by stirring up a muddy pool?

Let us deal first with the case of solutions. When sugar is dissolved in water there is an even distribution of sugar throughout the water; the same holds good for a colloidal solution of platinum in water. But when the two solutions are placed in dialyzers, it is found that the sugar will pass through the screen much more rapidly than the platinum is able to do. There is evidently a difference between the two kinds of "solution." Then again, when the ultra-microscope is applied to the two solutions, a marked difference is apparent: for in the sugar solution no separate particles can be detected; while the platinum sol appears to be full of minute floating bodies. Further, when the poles of an electric battery are dipped into a sugar solution to which a little acid has been added, the sugar is unaffected; while if the experiment be repeated with the colloidal platinum an examination with the ultra-microscope shows that the colloid begins to move towards one of the poles and to accumulate there. These differences indicate that there is a marked dividing line between the colloidal solution and the true solution, such as the sugar one. And it may be pointed out that the phenomenon of coagulation, to which we have already referred, is shown only by colloidal solutions and not by true solutions.

Turning next to the question of suspensions, we come to a more difficult problem. It is clear that colloidal "solutions" of metals, for example, are really suspensions of fine particles of the metal in, say, water; for, by means of the ultra-microscope, the number of these floating bodies can actually be counted, so that to this extent there is a similarity

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between a colloid solution and a suspension of earthy particles in a specimen of muddy water. But there are some properties possessed by colloidal solutions of metals which are not shared by ordinary suspensions. For instance, the compound of hydrogen and oxygen which is known as hydrogen peroxide has the property of decomposing slowly into water and oxygen. The rate at which it decomposes under a given set of conditions can be measured chemically. If to the hydrogen peroxide some colloidal platinum be added—a mere trace is sufficient—the rate of decomposition of the hydrogen peroxide is much increased. Now, a suspension of sand in water has not this effect upon the decomposition of the peroxide; so that in these two experiments it is clear that the colloidal platinum is an active agent, while the sandy suspension is a mere passive body. It has been proved that the difference is not due to the chemical nature of the platinum, for a piece of platinum does not affect the hydrogen peroxide's decomposition to any extent. It is evident that the colloidal condition has some effect which is not shared either by platinum in bulk or by a mere coarse suspension of sandy particles. A very convincing proof of this was furnished by Bredig, who proved that a colloidal platinum solution could be "poisoned" with prussic acid, and that in this way it could be deprived of its influence upon the hydrogen peroxide decomposition. No parallel case was observed with coarse suspensions: but it was discovered that a ferment called emulsin has properties exactly analogous to colloidal platinum in respect to hydrogen peroxide; and this ferment can be "killed" by the application of prussic acid. Thus the platinum solution resembles the ferment to a certain extent, and differs entirely from the simple suspension.

With regard to the peculiar action of colloidal platinum in this experiment, some information can be gained from a somewhat similar case in which platinum also plays a part.

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When sulphur is burned in air, we get the well-known "smell of sulphur," due to the compound sulphur dioxide which is formed. This substance contains one atom of sulphur and two atoms of oxygen. Now, under ordinary circumstances, this sulphur dioxide will not combine with any more oxygen, but if we pass a mixture of oxygen and sulphur dioxide over very finely divided and heated platinum, we find that combination of the two gases occurs, with the formation of sulphur trioxide, which contains one atom of sulphur and three atoms of oxygen. This reaction forms the basis of what is called the "Contact Process" of manufacturing sulphuric acid, for this last body is formed when sulphur trioxide is dissolved in water. If we pass sulphur dioxide and oxygen over a sheet of platinum instead of finely divided platinum, we find that the two gases do not combine with each other nearly so rapidly as they do when finely divided platinum is used. Evidently, then, the platinum has greatest effect in a state of fine division; and since in a colloidal solution the particles of platinum are exceedingly small, the metal is in the finest state of division possible.

Now, let us see how it comes that a metal is more active in a finely divided condition than in large masses. Suppose that we have a cube of platinum measuring two inches along each edge. The total area of platinum exposed is twenty-four square inches, and only these twenty-four square inches of platinum will come into contact with any reagent which we apply to the platinum. Next let us imagine that we cut this platinum cube into eight smaller cubes, each of which will measure one inch along the edge. The total area of platinum now exposed is much greater than before, for each cube has a surface of six square inches, so that the total surface now exposed is forty-eight square inches, or twice the surface of the original cube. Thus by dividing and subdividing a given mass of platinum we cause it to expose a

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greater and greater surface to the action of any external reagent; or, conversely, we allow a greater and greater surface of platinum to act upon a given reagent. It becomes clear that in the extremely fine state of division existing in a platinum sol an immense area of platinum must be exposed, and any action which the platinum is capable of exerting in a state of coarse division must be multiplied many times in the colloidal state, owing to the increase in surface of platinum which is free to act.

We have already mentioned the manner in which, by means of the ultra-microscope, it is possible to estimate the number of particles in a given colloid sol. But if we know, in addition to this, the mass of substance in the suspension, it is easy to see that we have only to divide the total mass by the number of particles in order to form a rough estimate of the average mass of the particles. This has been done in many cases, and it has been shown that in some colloidal solutions of gold in water, particles are found which have a diameter equal to ten thousand times that of a hydrogen molecule. From this it is clear that in these sols we are reaching down very near to the borderland of the molecular world.

No account of colloids would be complete without reference to what is known as the Brownian Movement, and we must therefore deal very briefly with this important phenomenon, which has led in recent years to much speculation. When a sovereign is dropped into water, the coin, being heavier than the liquid, falls to the bottom and remains there. Repeat the experiment with a half-sovereign and the result is the same. It might be concluded that any piece of gold, when placed in water, will sink at once to the bottom and remain there without showing any signs of rising again to the surface. This deduction, well founded though it seems, turns out to be incorrect when we reduce the size of the gold particle to a sufficient degree. Let us see how this discovery was made.

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In 1827, Brown, in the course of some botanical researches, observed that certain very minute particles, when suspended in a liquid, did not sink at once to the bottom, but instead of this they remained floating and were apparently actuated by some force which kept them in a state of continual motion, darting hither and thither in the liquid. This curious phenomenon has been named after its discoverer and is known as the Brownian Movement.

At first it was assumed that a motion such as this was comparable to the movement of motes in a sunbeam, and its cause was sought by analogy in currents in the liquid. Further examination, however, soon showed that this explanation was invalid. When the movements of motes in a sunbeam are examined, it is easy to detect, from their general trend, the direction of the air-current which produces them; but no such regularity in flow could be traced in the Brownian movement. Instead of moving in large groups, the particles in the Brownian movement show complete independence of each other; two particles extremely close to one another may move in opposite directions. Further, external influences appear to have much less effect upon the movement of the particles than might be expected. Thus, if we observe the motion of particles in a vessel placed on a solid masonry pedestal in the country, and make our observation at night in order to avoid the usual vibrations of the earth during the daytime, we find the movement just as brisk under these conditions as it is when we repeat the observations during the day in the heart of a town, amid all the shocks of heavy traffic. The effect of temperature upon the phenomenon has also been studied, and it has been found that the Brownian movement cannot be traced to the influence of convection currents set up within the liquid.

Brown, in the course of his investigations, had shown that the movement of particles is more rapid as their volume is decreased, and later workers proved that in a given liquid

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two particles of the same size would show the same degree of agitation no matter whether they were of the same material or not. The same holds good for their density, as it has been shown that a heavy particle moves as briskly as a lighter one, provided that the size of the two is the same.

Finally, as far as we can determine, the Brownian movement is unceasing; the machinery never appears to run down. It has been watched for months, and no cessation has been noticed; and it is found going on at the present day in liquids which have been enclosed in quartz at some far-off geological period. Apparently, then, it is spontaneous, incessant, and interminable, as far as we can see.

Now, colloidal solutions are really suspensions of very tiny particles, so that it might be anticipated that we should find the Brownian movement in action when we examine a sol. This has been proved to be the case, and observations on gold sols have shown that the particles of gold in this condition are actually in a state of incessant movement. From this it is evident that although gold in large masses such as a sovereign or a half-sovereign will fall to the bottom of a liquid such as water, yet when the gold is divided into sufficiently fine particles, these particles will remain suspended in the liquid apparently for an indefinite time.

Examination of the movements of these tiny particles suggested that the Brownian movement is not altogether irregular, but that instead the particles appear to oscillate about a mean position. Since the metallic specks are in very rapid motion, it might appear difficult to follow their movements with any degree of accuracy, but Svedberg has devised a most ingenious apparatus which allows us not only to do this, but even to obtain a permanent record of the motion. Suppose that we pour through the field of an ultra-microscope a coarse suspension whose particles do not show the Brownian movement. Each of these particles will be at rest with respect to the liquid surrounding it, but

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will be carried down in a straight line through the field by the flow of the liquid. Therefore when we look through the ultra-microscope at the stream we shall see, not single sparks of light, but a series of straight lines like the trails of rockets, each line representing the path of a particle across the field. Now, when we substitute for the coarse suspension a colloidal solution whose particles are in Brownian motion, we shall find that we no longer get a series of *straight* lines, but that we see a set of wavy lines, the waves being due to the movements of the particles about their mean positions; and the phenomenon can be permanently recorded by photographing the movement by means of a cinematograph. If we know the rate at which the liquid is flowing through the field, we can deduce the path of the particles in their Brownian movements in the plane of the ultra-microscope field, and the photographic plate gives us a permanent record of the whole phenomenon.

At the commencement of this chapter it was mentioned that the study of colloids was of importance, not only from the point of view of the chemist or physicist, but also on account of the fact that the subject is interwoven with many other branches of science and industry. We may, in concluding our survey of the colloid question, indicate one or two of these lines.

Of all subjects, mineralogy appears at first sight to be the one in which a knowledge of colloid properties would be of least assistance, yet in recent years it has been shown that there is an intimate connection between the two branches of science. Many of the common minerals have been shown to be colloid gels; so that knowledge acquired in other directions has added to the mineralogist's information with regard to the manner in which these substances have been produced and the properties which they exhibit. Thus the dendritic rocks, which show a peculiar feathery

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structure on their surface or within their body, have been proved to be formed by the diffusion of one colloid into another; and the tints of some minerals have been traced, by the ultra-microscope, to the presence of fine particles within their structure, similar to those specks found in the stained glasses. The coloration acquired by certain minerals when submitted to the action of radium has a like origin; for under the influence of radium fine metallic particles are liberated within the mineral, with the result that the material becomes discoloured, just as glass does under the action of radioactive materials.

In the domain of agriculture, also, a knowledge of the properties of colloids is rapidly bearing fruit. This is so not only because plants are largely built up from colloid substances, but also because the soils on which they grow are found to contain large proportions of inorganic colloid gels whose properties play a great part in the operation of manures; while the same may be said of the colloidal humus substances whose presence is desirable if a good crop is to be obtained.

In medicine, colloidal drugs are now beginning to be utilized, for it is found that gold, silver, and mercury can be introduced into the body in the form of colloidal solution with very good effect.

When we turn to the application of colloids in industry, we find that much has already been done; and in the future it seems probable that many further uses of colloids will be discovered in this field. Mention has already been made of the light which the study of colloids has thrown upon the stained glass industry; and the processes by which mirrors are produced depend in part upon the formation of colloidal solutions of silver. Pottery, cement, paste, glass, and celluloid all touch upon the colloid question, while the extraction of sugar from the sugar-cane depends upon the dialysis of the crystalloid sugar through the walls

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of the plant's cells in which the colloidal albuminous products are retained. The ordinary photographic dry plate is coated with an emulsion of silver salts, and many of the problems connected with the manufacture of sensitive plates and papers turn upon colloid properties. The great rubber industry is one in which the material dealt with is itself a typical colloid.

These few examples suffice to show the widespread importance of any knowledge gained in this field, and they give the clue to the sudden rise of the chemistry of colloids as soon as the ramifications of the subject became clear.

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CHAPTER VI

THE WORK OF THE SPECTROSCOPE

CONSIDERATION of the methods of the experimental sciences discloses a somewhat peculiar phenomenon with regard to our methods of measurement. In scientific work accurate determinations of many different kinds are frequently necessary, and reflection will show that all our efforts in this direction are tending to make our visual sense the final factor in the problem. Thus we have nerves which tell us whether a body is hot or cold, but we do not rely upon them; instead, we construct a thermometer and gauge the temperature by the rise of mercury which we see taking place in a tube. We have muscles which can give us an idea whether a body is heavy or light, but instead of utilizing these, we employ a balance and read off our results on its pointer. Again, our nerves can inform us of the passage of some electric currents, but when we wish to have an accurate knowledge of these electrical phenomena we press volt-meters into service, and again have recourse to our optic nerves. Our hearing has a very limited range, and a chronograph tracing of a vibrating tuning-fork is a much more accurate method of measuring its vibrations than is furnished by the human ear. Taste and smell are the only two senses for which no visual substitute has yet been contrived.

Seeing this preference given to the human eye over all the other sensory machinery of the body, an observer might

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conclude that the optical machinery of a man was an exceedingly delicate and comprehensive apparatus; but in point of fact this is not by any means the case, as a consideration of the actual range of human vision will show. The oscillations of the ether which our optic nerves perceive as light are not isolated; on the one side they merge into the ultra-violet waves, while at the other end of the scale are the infra-red vibrations, and beyond them, again, the Hertz waves. Take the extreme end of the spectrum known to us, where the waves are shortest. In this ultra-violet group the wave-lengths lie between 0·000004 inch to 0·000016 inch; that is to say, that it would require about a quarter of a million of the shorter waves to extend over one inch. Next comes the visible region of the spectrum. Here the most rapid vibrations are in the violet, where the wave-length is about 0·000016 inch; but as we pass up the spectrum the vibrations become slower, until, having run through blue, green, yellow, and orange, we come to the other end of the visible spectrum, where, in the red, the wave-length has increased to 0·00003 inch. But this does not bring us to the end. Beyond the spectrum visible to our eye we find a spectrum of heat-rays, called the infra-red region, which comprises wave-lengths between 0·00003 inch and 0·003 inch. This last point brings us to the end of the continuous spectrum as we know it. A gap extending over a region of wave-lengths lying between 0·003 inch and 0·1 inch comes next. Then beyond that begin the electric vibrations which are used in wireless telegraphy, and which are named, after their discoverer, the Hertz waves. The table on the following page will give a conspectus of the present state of our knowledge of the complete spectrum.

A glance at this table shows how very limited is the range of the human eye, bounded as it is on one side by waves a hundred-thousandth of an inch in length, and on the

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		Wave-length in inches.	
Visible spectrum	Most rapid vibrations detected photographically	0·000004	Range of photographic spectrum.
	Violet end of the visible spectrum	0·000016	
	Red end of the visible spectrum	0·00003	
	Slowest vibrations detected photographically	0·00008	
	Slowest vibrations detected in heat-ray region	0·004	End of bolometer spectrum.
	Gap in spectrum where our present apparatus fails to detect waves	—	
	Slowest Hertzian waves	0·1	Electrical spectrum begins.

other by those vibrations, three hundred thousandths of an inch long, which produce in our machinery the sensation of red light. And now we come to one of the most curious paradoxes of science. Owing to our habit of referring all measurements where possible to our visual sense, we seek some method of "seeing the invisible." Stated in this way, the matter seems absurd; but it is actually a description of the process utilized. We cannot, with our limited optic nerve, appreciate these fine vibrations in the ultra-violet region, or these ampler periods in the infra-red; but we can devise apparatus which will be affected by them, and from the influence of the vibrations upon our apparatus we are enabled to appreciate and even measure the invisible portions of the spectrum.

Thus, even when we screen off all the visible part of sunlight, we find something left in the rays which affects certain silver salts chemically; and by utilizing this chemical process in a photographic plate we get a measure of the work done by the "invisible light" which is present in the rays of the sun, but which lies outside our visual range. Again, at the other end of the visual spectrum, we find heat effects taking place which we cannot detect by means of the optic nerve direct. But by using the bolometer, which is a very delicate thermometer, we are able

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to see the influence of the heat-rays and measure the frequency of their oscillations.

When a beam of light is passed through the prism of a spectroscope, all the different vibrations are sifted out from one another, so that instead of all of them impinging simultaneously upon our eye, we can examine each wave-length separately. Let us see what results are obtainable in this manner.

Spectra may be divided into two main classes: emission spectra and absorption spectra. For the present we may take up the question of emission spectra, dealing later with the spectra resulting from the absorption of light.

When the light from a luminous source is examined spectroscopically, the spectrum may appear under either of two forms: there may be a continuous background of light in the field of the instrument, or the observer may find merely a series of fine lines before him. Examples of the two types are shown in Plate I., where the spectrum of the acetylene lamp is continuous, while that of an electric arc passing between carbon electrodes shows a multitude of very closely placed lines. Sometimes these lines are so numerous and closely associated that they appear as bands of light or flutings in the spectrum.

Research has shown that these differences in spectra may be easily explained. An incandescent solid or liquid gives rise to a continuous spectrum because it emits light vibrations whose wave-lengths are of many different lengths though each differs but little from its next longer or shorter neighbour: so that when the light is analyzed in the spectroscope it is spread out into a practically continuous band; there are no gaps in the series of wave-lengths which would give rise to dark divisions in the system. In the spectra of vapours, on the contrary, the light vibrations are not so evenly spread over the whole range; some wave-lengths are

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missing from the scale, so that the spectrum appears as a series of lines separated by dark spaces.

The difference in appearance between the spectrum of the acetylene lamp and that of a spark passed between two carbon electrodes will now be clear. In the acetylene flame, which is a very sooty one as most people know, particles of carbon are heated to a high temperature. They become incandescent, and it is to them that we owe the brilliance of our acetylene lamps. Thus, since the temperature of the flame is not sufficiently high to convert carbon into vapour, we get the spectrum of an incandescent solid. A similar effect is produced when lime is heated with an oxy-hydrogen blowpipe during the production of limelight.

In the case of the electric arc or spark, however, we are dealing with temperatures incomparably higher; some carbon particles are torn from the electrodes during the passage of the current and are volatilized in the intensely heated focus between the poles. The resulting spectrum, therefore, shows the series of lines characteristic of a highly heated vapour.

An examination of Plate I. will bring out another point of interest. Comparison of the spectra of sodium vapour, helium and carbon vapour shows that they do not resemble each other. Sodium vapour emits very few lines, only one of which appears on the plate; helium's spectrum is rather richer; but neither of them can be compared with the carbon spectrum. Clearly, then, each element has spectral peculiarities of its own.

Now, at the present day the spectra of almost all the known elements have been very carefully mapped and the positions of the most important lines in them have been accurately determined. Thus, if the presence of an element is suspected in a substance, all we have to do is to examine the spectrum of the body, and then endeavour to identify the lines in the spectrum which are characteristic of the element which is supposed to be present. Plate I. gives the spectrum

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of a mixture of copper and carbon taken with short exposures; the spectra of the two individual bodies are shown above and below the spectrum of the mixture, and the reader will have no difficulty in identifying some of the characteristic lines of each element among the lines of the mixture's spectrum.

This is an example of one field in which the spectroscope is utilized in chemistry; it furnishes a ready means of discovering the constituents of a mixture. But in connection with this another point arises. Suppose that in the examination of a body's spectrum we find lines which do not correspond to any element with which we are already acquainted. Evidently we should at once suspect the presence of a hitherto unknown element in the body under investigation, and, as we shall see in the chapter upon the inert elements, most of these bodies were first detected by the aid of the spectroscope. Again, when the spectroscope is turned upon the stars, spectra are observed which do not correspond to anything which has yet been isolated on the Earth, so that there are evidently still further elements in the universe beyond those which we have handled in our laboratories. Helium furnishes a case of some interest; for the spectrum of this body gives certain lines occurring in the solar spectrum, and the element had actually been named before it was obtained from terrestrial sources.

In addition to the ease with which it can be applied to the study of mixtures, the spectroscope has an especial advantage over ordinary methods of analysis when we come to deal with very small quantities of substances. By means of the spectroscope it is possible to prove the presence of one part of sodium vapour in twenty thousand million parts of air, which corresponds to our being able to detect a fragment of common salt weighing only 0·00000000001 oz. This is a refinement of testing to which parallels are not easy to find. When it is recalled that the most delicate

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balance ever constructed (that of Ramsay and Gray, see p. 191) deals with quantities such as 0·00000004 oz., it is evident that the spectroscope is about four thousand times more sensitive than the balance.

We have seen that the nature of the source of light has a considerable influence upon the character of the spectra produced by various bodies. Let us examine this point more closely and see what general results have been obtained. The usual sources of light employed in practice are four in number: flames, electric arcs, electric sparks, and luminous vacuum tubes. We may take these up in turn.

The spectra of substances heated in a flame may be obtained in various ways. For example, a fragment of the substance to be examined may be placed upon a piece of platinum wire and held in the flame of a Bunsen burner. Platinum does not volatilize at this temperature, so that the flame is coloured only by the substance under examination, provided that it is volatile at the temperature of the Bunsen burner. Sometimes, owing to the easy fusibility of the specimen, it is impossible to keep any of it attached to the wire, as it drips away as soon as heat is applied. In this case other means have to be employed: air charged with fine particles of the body is blown into the flame, or a solution of the substance is allowed to trickle on to a piece of platinum placed in the centre of the hot region. These flame spectra may be of two kinds, either continuous or discontinuous. We have already seen that incandescent carbon gives a continuous spectrum, while sodium in the Bunsen flame produces only a few lines.

The temperature of the hottest region in the flame of a Bunsen burner is roughly 1800° C. When we turn to the electric arc we find a great rise in the scale, for one electrode in a carbon arc ranges round 3000° C., while in the neighbourhood of the other electrode the temperature may rise to about 4000° C. Now, when the spectrum of such an arc is

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concentrated upon the slit of a spectroscope, an examination of its spectrum shows that it is made up of a series of lines. Further inspection reveals the fact that these lines are not all of the same length; some traverse the whole field in a vertical direction, while others do not extend the full length upwards and downwards, but are confined to the central region of the field. How can we explain this phenomenon? From what has been said with regard to the difference in temperature at the two electrodes, it is clear that the temperature of the arc is not uniform; some portions are hotter than others. The central horizontal axis of the spectrum as seen in the spectroscope represents the spectrum of the hottest portion of the arc; in it all the possible vibrations are stimulated to their utmost extent. In the cooler portions, however, the temperature may not be sufficiently high to produce the vibrations requisite for the emission of light, and consequently the ends of the lines do not appear. A further point of interest is found in the fact that the long lines are not of even breadth throughout; they tend to a very elongated cigar shape, being thicker in the centre and thin at either end. Now, it is obvious that when we are dealing with a substance which is hard to volatilize, the hottest portion of the arc will contain the greatest proportion of vapour, for in the cooler portions condensation will take place to some extent. Thus the broadening of which we have spoken may reasonably be ascribed to the increase in the density of the luminous vapour at the hottest region of the arc, as compared with the lesser density of the gases in the cooler regions. The length of the lines, then, gives us, as Lockyer pointed out, a means of comparing the influence of different temperatures upon bodies: those which give long lines are evidently capable of emitting light at lower temperatures than the other substances which give lines whose length indicates that they glow only in the hottest regions of the arc.

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Coming next to the electric spark, which is obtained from an induction coil and condenser, we again reach a much higher temperature.[1] Exact measurements are not possible, but it may be taken as a rough estimate that in the spark the temperature approaches 12,000° C. In view of this great difference in temperature between the arc and the spark, we should expect to find that a considerable difference made its appearance in the spectrum when the one source of illumination is used instead of the other. This actually proves to be the case. When the spark is substituted for the arc, a host of new lines make their appearance in the spectrum, while at the same time certain parts of the arc spectrum become modified in the spark spectrum. The problem is not quite so simple as it appears at first sight, however. Various factors have been suggested which might come into play in addition to a mere temperature effect. Thus, the increase in temperature beyond a certain point might lead to deep-seated chemical alterations in the bodies under examination; at the high temperature of the spark a dissociation of the elements might be possible; so that beyond the physical effects of a rise in temperature we might have chemical influences to take into account. Then, again, there are electrical disturbances taking place in both arc and spark, and the differences between them might play some part in the alteration of the spectra. There is also some experimental evidence bearing on the point which throws considerable doubt upon the hypothesis that the difference between arc and spark spectra is merely one due to an alteration in temperature. It has been shown that when an arc is burned in an atmosphere of hydrogen instead of in air, its spectrum approximates to that of the spark spectrum of the electrodes; the same phenomenon is observed when the arc is burned under water; and a similar result is obtained when the strength of the current feeding the arc is diminished. Now,

[1] Some authorities contest this view.

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in each of these three experiments the temperature of the arc is actually being lowered instead of being raised, yet we get results approximating to those obtained with the spark discharge, which is supposed to be three or four times as hot as the arc discharge. This example will serve to show how complex an apparently simple problem may become in the light of further evidence. At present we have no full explanation before us.

The last source of light which we need mention is obtained from tubes of gas under low pressure when they are excited by an electric current, which is passed across a gap between two metallic electrodes fused into the glass of the tube. By employing a device of this type it is possible to obtain the spectra of gases which could not easily be dealt with in any other manner.

A comparison of the spectra of various elements produced by two or more of the above methods brings to light the fact that every element exhibits a characteristic spectrum of its own. Thus, the spectrum of sodium, whether it be obtained from a flame, an arc, or a spark, always contains the characteristic line shown in Plate I. On the other hand, as we have seen, the number of the lines may increase with a rise in the temperature of the vapour employed, or by some other alteration in the source of light. For instance, if a lithium salt be placed in a Bunsen flame, its spectrum will be found to consist of a bright red line and a faint orange line; but if we examine the spectrum of lithium when heated in an oxyhydrogen flame (which is much hotter than that of a Bunsen burner), we find that about twenty additional lines are visible.

With regard to the spectra of compounds, these consist of bands of various widths; but it is often very difficult to obtain them, for as soon as a compound is heated to a temperature sufficiently high to enable it to emit light, the compound may dissociate into its component atoms, and

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we get merely the spectra of its various elemental constituents instead of the banded spectrum of the compound itself. For example, when a sodium or a lithium salt is placed in a flame, the spectra observed are those of the elements sodium and lithium, and not those which would characterize the salt.

So far in this chapter we have dealt with emission spectra, those which are sent out by bodies when excited by heat or electricity. We must now turn to examine a different type of spectra, those which are observed when portions of the spectrum are absorbed during the passage of light through various media.

A very simple example will make clear the nature of absorption spectra, as they are termed. If we examine an acetylene lamp, we see that it emits what we term white light, and the photograph of its spectrum, reproduced in Plate I., shows that this light consists of a whole series of oscillations extending throughout the visible spectrum, and containing no gaps in its continuity. Suppose now that we interpose a piece of red glass between our eye and the lamp flame, we then see through the glass the image of a red flame. Obviously the red glass has cut off from the complete spectrum all the rays except those which produce on our optic nerve the sensation of red light. If we use a blue glass instead of a red one, we should see only the blue rays from the light source; so that the acetylene flame would appear blue in tint. These experiments show that it is possible, by the interposition of certain substances, to cut off certain rays from the spectrum while allowing others to pass through the medium without alteration. In other words, the light has suffered what is called selective absorption.

Another experiment throws further light upon the problem. Suppose that a long tank is filled with red ink, and that a light is examined by looking through the tank. An observer would notice that the light appeared to be red; but

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if a much shorter tank were employed the light would not appear so red, and if we made the tank sufficiently short, until only the merest film of red ink interposed between the observer's eye and the light source, it is probable that finally he would not be able to detect the red colour at all. From this it is clear that the amount of light absorbed will depend upon two factors: the brightness of the source of light and the thickness of the absorbing layer which is interposed between the light source and the observer.

In the scientific examination of absorption spectra very little work is now carried out upon solid substances, as the technique is clumsy. Instead of using plates of solid materials, solutions are employed wherever possible, as they are much more easily handled. A concrete example will make the matter simpler. Suppose that we have a tube one inch square at the end and ten inches long. This is filled with water, and in the water we dissolve ten grammes of the substance whose spectrum we wish to examine. Each end of the tube is then closed with a plate of glass. We next photograph the spectrum of a light source by means of the spectroscope, and during the exposure we interpose the tube of solution between the slit of the spectroscope and the source of light in such a way that the light must pass down the whole length of the tube before reaching the slit. The accompanying figure (Fig. 5) will give an idea of the apparatus. E, E are the electrodes of an arc which is used as the light source. C is the tube containing the

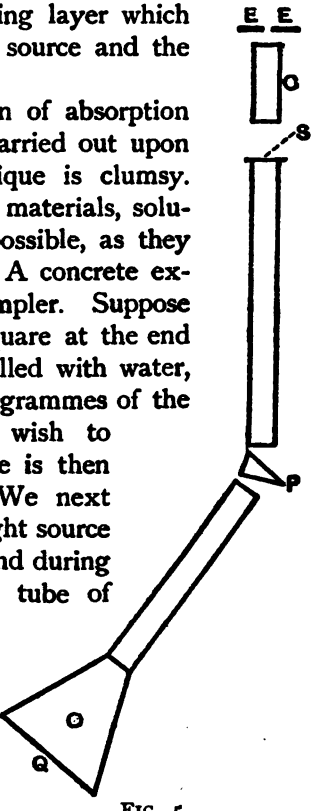

FIG. 5.

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solution. S is the slit of the spectroscope, P its prism; O is a camera so arranged that the image of the spectrum falls upon the photographic plate Q. A photograph is taken through the ten-inch-long tube. Now suppose that one inch is cut off the tube and that the glass plate is replaced on the end of the new tube, which is now only nine inches long. When we photograph through this layer, the light from the source will be passing through only nine-tenths of the thickness of solution which it traversed in the first exposure; and consequently the rays will meet in their passage only nine-tenths of the quantity of the solute which they had to pass through in the previous case. If we reduce the length of the tube further, we get a steady diminution in the amount of dissolved substance which the light has to pass through at each exposure. In fact, the result is the same as if we had employed a plate of the solid substance and had shaved a layer off it before each new exposure.

Now let us see what results we should get upon our plate when we had developed it, assuming that at each exposure we moved it down a little so as to expose a fresh surface to the light. Plate II. is a reproduction of an actual photograph taken in this way. At the side are the thicknesses in millimetres of solution through which the light was passed. It will be noticed that the thicker layers allow much less light to pass through than do the thinner layers; and that finally the whole length of the iron arc's spectrum is able to penetrate the thinnest layer of solution. Further examination will show that the light is cut off in two ways. In the thicker layers, the whole of the ultra-violet spectrum (the left hand of the plate) has been absorbed. This is called general absorption. Then as the layers become thinner, some light struggles through at a point corresponding to a wave-length of 2600 as can be seen from the scale. Further thinning of the layer of solution results in more

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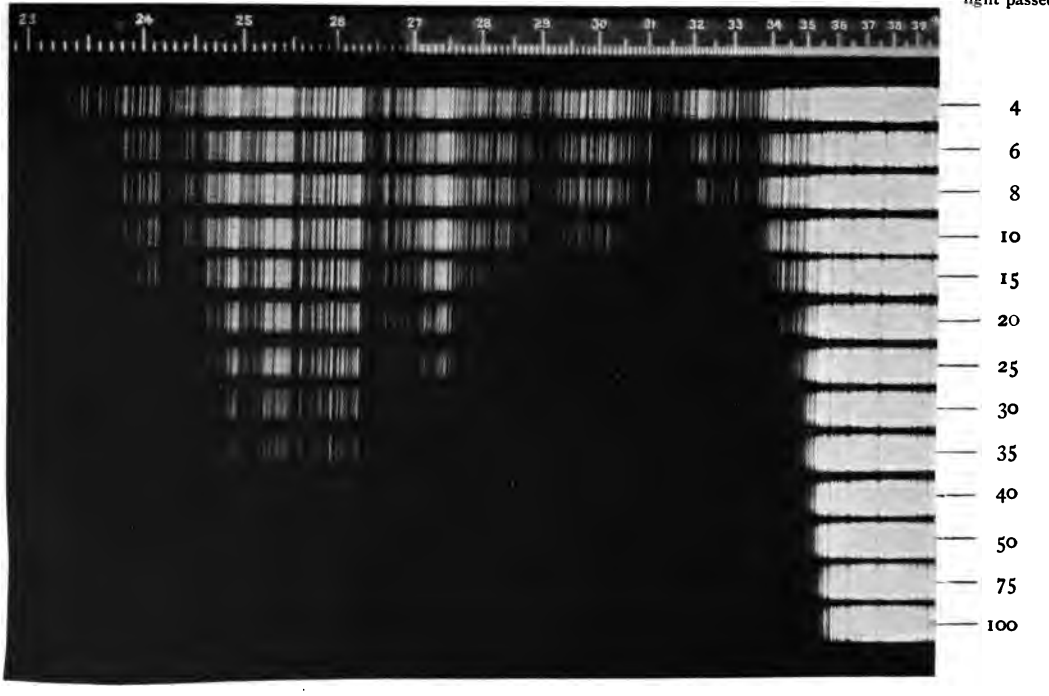
PLATE II



AN ABSORPTION SPECTRUM. THE SCALE IS ONE OF WAVE-LENGTHS.

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light coming through at this point, so that the light broadens out into a triangular formation. Thus at a thickness of 25 millimetres we find that light is transmitted up to wave-length 3450; then comes a gap where all the light is absorbed up to wave-length 2750; and finally light comes through again. This is what is termed selective absorption; there is discontinuity in the absorptive power of the solution, and light of wave-lengths both longer and shorter than the absorbed vibrations is permitted to pass unobstructed.

The black wedge of absorption which is bounded on its two sides by the transmitted spectrum is termed an absorption band, and the extreme tip of the wedge, which occurs in the plate at the wave-length 3150, is called the head of the band. Now when the absorption spectra of various substances are examined in this way, it is found that they differ from each other in a very marked manner. It may be that the head of the band occurs at a different wave-length in each case, or the shape of the band may be different, some being narrow and deep, others being broad and shallow; or, again, the thicknesses of solution in which the band makes its appearance may not be the same in two cases. Each particular substance has its own characteristic absorption spectrum, just as each element has a peculiar emission spectrum of its own. Thus absorption spectra can be utilized in analytical work just as emission spectra are employed, but the former have the extra advantage that we can obtain the absorption spectra of compounds whose emission spectra cannot be observed owing to the dissociation of the compound under the conditions necessary in a light source.

The major part of the work upon absorption spectra in the laboratory is concerned with the spectra of compounds; but elements also can be made to yield similar spectra. Thus sodium, as we have seen, emits a spectrum consisting

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of two[1] yellow lines. Suppose, now, that a source of light producing a continuous spectrum be employed, and that between this light source and the slit of the spectroscope a layer of sodium vapour be interposed; it is found that a black line takes the place of the sodium line in the spectrum. This black line represents the absorption spectrum of sodium vapour. When highly heated, the sodium atom is thrown into a state of vibration, and the intra-atomic oscillations set up waves in the ether which have a certain period of vibration; now, if the sodium be not heated and waves of light of this particular wave-length are allowed to traverse the sodium vapour, the intra-atomic particles are tuned to that particular rate of vibration, and they consequently sift it out and prevent its passage through the molecule, so that a black gap corresponding to the absorption occurs in the spectrum.

A few of the applications of the spectroscope in practice may now be taken up. Reference has already been made to its uses in analysis and in the detection of unknown elements; but there are wider fields in which it is the only instrument which can help us. For example, by attaching a spectroscope to a telescope, it is possible to map the spectra of the stars, and in this way to ascertain something of the composition of the heavenly bodies. The solar spectrum presents certain phenomena which deserve a fuller description. When a beam of sunlight is directed upon the slit of a spectroscope, the spectrum is found to consist of a bright band of light extending over the whole visible spectrum from red to violet; but crossing this band at right angles run a very large number of dark lines. These lines are always observed whenever the solar spectrum is examined, and their relative positions remain unaltered. It was at first suggested that these lines owed their origin to something in the terrestrial atmosphere; but this idea

[1] The single line shown in Plate I., when examined in more powerful instruments, is found to consist of two very closely neighbouring lines.

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was shown to be untenable by Fraunhofer, who proved that the fixed stars had lines in their spectra which were not identical with those mapped by him in the solar spectrum. Let us trace back the path of a ray of light from our instrument to its source in each case. The ray of sunlight has passed through our atmosphere, through outer space, through the atmosphere of the sun, and has emanated from the solar surface. On the other hand, the starlight has traversed our atmosphere, outer space, and the stellar atmosphere, emanating as it does from the surface of the star. Black lines do not occur in pure emission spectra, so we may rule out of consideration the idea that these black lines originate in any phenomena taking place in the glowing part of the sun or of the star. Again, since the light from both star and sun has traversed outer space and the terrestrial atmosphere, if either of these had the power of producing the dark lines the same lines would appear in both solar and stellar spectra. The only two other factors are the stellar atmosphere and the solar atmosphere. Thus we may safely conclude that the dark lines in the solar spectrum take their rise in the atmosphere of the sun.

Kirchhoff was the first to demonstrate the manner in which these lines are produced. On comparing the emission spectra of certain metals with the solar spectrum, he found that where a bright line occurs in the emission spectrum of calcium there is a corresponding dark line in the solar spectrum; and from what we have already seen with regard to the absorption spectrum of sodium vapour, it will be clear that these dark lines in the solar spectrum are really the absorption spectra of some elements in the atmosphere of the sun. When the emission spectra of certain elements were brought side by side with the solar spectrum in the field of the same spectroscope, it was found that each bright line had its counterpart in the dark solar line. Careful examination in this way has proved the presence in the

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solar atmosphere of between thirty and forty terrestrial elements.

Another application of the spectroscope to astronomical problems has resulted in very important results. It is a well-known fact that during a total solar eclipse the disk of the sun is found to be surrounded by wisps of light which are termed the solar prominences, and are now known to be vast jets of burning gas projected upward from the solar surface. Under ordinary conditions it is impossible to observe these prominences, for the intense light of the solar disk so dazzles the eye that it is incapable of perceiving the fainter illumination of these prominences. The application of the spectroscope, however, has made it possible to inspect these jets of gas even at midday, and to sketch their forms in perfect comfort.

It will be recalled that when observed in an ordinary spectroscope the solar spectrum appears as a continuous band of light. This continuous strip, however, actually contains a multitude of fine lines due to the emission spectra of the various elements in the sun. Suppose that in a certain spectroscope this band is two inches in length. Now imagine that another spectroscope is built so that the spectrum is spread out over a longer band, say four inches in length. If the amount of light which passes through the slits of the two spectroscopes is exactly the same, it is clear that the four-inch band will be only half as bright as the two-inch band. Thus by increasing the dispersion of the spectroscope which we use we can get a longer band of light, and consequently this band will be less intensely illuminated, so that with a sufficiently long dispersion we can observe the spectrum without dazzling our eyes. But this process does not mean that we have weakened the brilliancy of any single line in the spectrum; we have merely spread the lines farther apart. Imagine that we had ten candles placed very close together; their light

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would appear as a bright spot which would tire our eyes; but if we moved the candles into line with a yard separating each candle from the next, we could look from candle to candle without feeling our eye over-tired. Each candle is as bright as ever, but the collective effect of the whole ten no longer affects our optic nerve. So it is when we lengthen the band of the solar spectrum; each line is as bright as ever, but each can be inspected singly without the dazzling effect which is found when all the lines are close together.

The slit of the spectroscope must also be made broader, for just as we cannot see a wide expanse of landscape through a deep-set narrow window, neither can we get the whole image of a prominence into view through a narrow slit in the spectroscope. One particular line in the spectrum of the prominence is now inspected; and it shows the form of the prominence brilliantly defined against the dimmer band of the continuous solar spectrum. In this way it is possible to observe the prominence for hours at a time without becoming dazzled, and the changes in form of these vast clouds of hydrogen gas may be mapped with the greatest ease.

These examples will suffice to show the application of the spectroscope to the detection of the constituents of the sun and stars; but there is another field of astronomy in which the same instrument has been pressed into service in a somewhat different fashion. The matter is a complicated one, and recourse must be had to analogy to make it clear. Imagine that a person is standing on the seashore, and that he finds the waves are breaking on the beach at the rate of one per second. So long as he stands still, one wave will break over his feet at each second; but if he begins to walk into the sea, he will find that the waves appear to break more frequently over him; and the faster he moves, the more waves he will encounter in each second.

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On the other hand, if he now turns and retreats toward the shore, the waves will appear to come slower, and intervals longer than a second will elapse between the break of each wave over him. In other words, the frequency of the waves will apparently increase as he walks against them, and decrease as he retreats before them. Turn now to sound. The whistle of a railway train standing in a station will appear to have a constant note to an observer placed further along the line. If the train, however, instead of standing still, is approaching the observer at high speed, the note of the whistle will appear to be higher up the scale as the train approaches, while if the train be running away from the observer, the note of the whistle will appear to be lower in the scale. In the former case more waves of sound are reaching the observer in each second, while in the latter there is a diminution in the number of waves per second as the train recedes. Now apply the same reasoning to the case of light. Suppose that we have a stationary body emitting light of a constant wave-length. When examined in the spectroscope this light will not vary. On the other hand, imagine that the light-source is approaching the spectroscope very rapidly; more waves of light per second will reach the instrument, and hence we shall appear to have a higher frequency of vibration. The light will appear to pass up the scale from the slow vibrations of the red end towards the more rapid vibrations of the violet end of the spectrum. On the other hand, if the source of light is moving away from the spectroscope, the waves will appear to come less frequently, and the colour will change from violet towards red. This case is often observed in stellar spectra. Careful examination of a star's spectrum at intervals shows, perhaps, that the star is becoming more and more violet in tint; for example, the hydrogen lines may shift slightly in position toward the violet end of the spectrum. This indicates that the star is coming

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towards us. If the spectrum changed from violet to red, the conclusion would be that the earth and the star were travelling away from each other. It is possible by this means to calculate the approximate speed at which star and Earth are approaching to or receding from each other, and in this way the problem of stellar motion in the line of sight is solved, a problem which no other means could decide for us.

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CHAPTER VII

CHEMISTRY IN SPACE

WHEN two compounds are examined and found to display totally different physical properties, it is natural to assume that their chemical compositions are not identical. Thus potassium bromide, which contains one atom of potassium united with one atom of bromine, has properties quite different from those of common salt, which contains one atom of sodium united with one atom of chlorine. In the earlier days of chemistry, this idea stood the test of experience very well; but as time went on it was found that compounds existed whose compositions were the same though their properties showed marked differences. Such substances were termed "isomeric compounds."

The explanation which suggested itself for such phenomena was very simple. Since it is clear that the same set of atoms occurs in the two isomeric compounds, the only difference between the two bodies must lie in arrangement of these atoms within their respective molecules. A rough example will make the matter clear. Suppose that two isomeric substances exist, each of which contains the atoms A, B, and C. If we try to imagine how these atoms are held together in the molecule, we can see that there are various ways in which they might be arranged. Thus in one case A might be joined to B, and B in turn united with C; or A might be joined to C and the latter in turn united with B. To express this idea, a dash may be employed to

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represent a bond of union between the atoms, so that the two arrangements would be represented thus—

A—B—C A—C—B

Now, the test of such an hypothesis is to push it to its furthest point and see if it suggests a new line of investigation. It has been assumed above that two isomeric compounds are known containing the atoms A, B, and C; but a little thought will show that there is at least another possible arrangement of these atoms, for the atom A might be used as the central atom of the group, thus—

B—A—C

If our initial hypothesis were correct, then, there should be a possible third isomeric compound containing A, B, and C, whose properties would be different from those of the other two, and investigation would demonstrate whether there was any truth in this supposition or not.

In actual practice, many such compounds, whose existence has been predicted theoretically, have been synthesized in practice, and the idea of chemical structure, as it is termed, is one of the most solidly founded theories in our modern chemistry.

Just as the theory of chemical structure seemed to be definitely established, however, it received a severe blow from the experimental side of the science. Various compounds were discovered which by no possible ingenuity could be accounted for on the current view of chemical structure. So long as the number of isomeric compounds known was less than that which had been theoretically predicted, it was possible to say that further research would result in the discovery of the missing substances; but when, as proved to be the case, a number of isomeric bodies was found greater than could be explained by the structural hypothesis, it became clear that the current views were

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insufficient, and a new advance became necessary. The present chapter will give a brief outline of the course of this development in chemistry.

In order to approach the subject in the simplest manner it will be necessary to deal with the phenomena of polarization. In a ray of *ordinary* light, the vibrations of the ether take place in all directions in a plane perpendicular to the direction in which the light is transmitted, but in a beam of *polarized* light these vibrations are confined to one line in this plane.

A consideration of the phenomena known in connection with tourmaline will make the matter clearer. Tourmaline is a transparent mineral of a very finely foliated structure. If two thin plates of tourmaline be held parallel to one another, light will pass them; but sliding one round on the top of the other, while still keeping them parallel, an increase or decrease in the amount of light passing through may be produced. It is found, further, that at one point in the revolution, no light at all comes through, while the position of greatest illumination is obtained by revolving one of the plates through 90° from the position of total darkness.

A very rough analogy will help to make the point easier to grasp. What actually happens is that the ray of light is polarized by the first tourmaline plate. Now suppose that instead of two tourmaline plates there are two books, the foliation of the tourmaline will be paralleled by the leaves of the volumes, and the ray of incident light may be represented by a pencil. It is clear that the pencil cannot be pushed between the pages of the first book without disturbing the leaves to a considerable extent, but if the pencil could be whittled down into a flat object like a paper-cutter it would then pass easily between the leaves. This whittling down of the pencil corresponds to the polarization of the light, in which all the rays except those in one particular plane are eliminated. Let a paper cutter be

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passed into the first book to represent the ray of polarized light. Now, if the leaves of the second book be brought parallel to those of the first (Fig. 6), the paper-cutter will pass through the second book easily, whereas if the leaves of the second book be placed at right angles to those of the first, the paper-cutter will refuse to pass into the second book (Fig. 7).

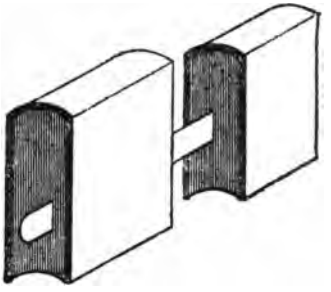

FIG. 6.

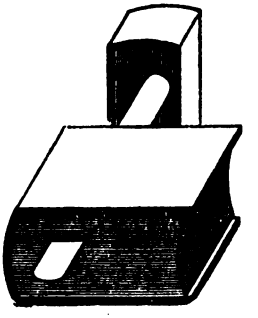

FIG. 7.

Now, suppose that two plates of tourmaline are so placed with respect to each other that the maximum amount of light passes through them without obstruction. If a layer of water be interposed between them, no change in the intensity of the transmitted light is observed; but if a slip of quartz be substituted for the water, a new phenomenon makes its appearance. It is now found that in order to get the maximum illumination one of the tourmaline plates must be revolved through a certain angle; and a little consideration will show that this must be due to a rotation of the plane of polarization. In passing through the quartz, the light which emerged from the first tourmaline plate has been twisted round, so that in order to get the same results as before, the second tourmaline plate must be turned through the same angle as that through which the light rays were rotated.

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This property possessed by quartz is termed optical rotatory power, and objects like quartz are called optically active bodies. Further investigation showed that, though crystalline quartz has this property, the power of rotating polarized light is lost when the quartz is reduced to the condition of amorphous silica. Evidently, then, the optical rotatory power of quartz is due merely to its crystalline form.

The researches of Biot and Seebeck in 1815 threw further light upon the subject. They discovered that certain substances like turpentine in the liquid state were capable of rotating the plane of polarization; and as in their experiments no crystals were employed, it was clear that some bodies had an optical rotatory power apart altogether from their crystalline form. Thus in the case of quartz it is the crystalline form which lends the power of rotation to the compound, but in the case of turpentine the source of optical rotatory power must be sought in the structure of the molecules of the compound and not in its physical conformation.

At this point, a new line of research impinged upon the previous work. Pasteur, in 1860, began an examination of the crystalline forms of various isomeric substances. Tartaric acid and racemic acid have the same composition, yet their properties are not the same; for the one body rotates the plane of polarization, while the other has no influence upon polarized light. An examination of the crystals of some salts of tartaric acid showed Pasteur that the crystals of tartaric acid differed from those of racemic acid; and, further, that the crystals of tartaric acid occurred in two varieties. The ends of the three types of crystal are illustrated in Fig. 8, and an examination of the diagrams will show that there is a certain relation between the three crystalline forms.

The racemic acid crystal is what is known as a holohedral

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crystal; it presents the full number of faces requisite to show a complete symmetry. In the crystals of tartaric acid, on the other hand, only half the number of faces occurring in the corresponding racemic acid crystal are developed, those marked in black being what are termed hemihedral faces. Closer inspection will bring out another curious point. It will be seen that the two tartaric acid crystals are related to one another as an object is related to its image in a mirror. The black hemihedral facet occurs in the top left-hand corner of one crystal and in the top right-hand corner of the other.

Racemic and salt (holohedral crystal).

Right- and left-handed crystals of tartaric acid salts (hemihedral crystals).

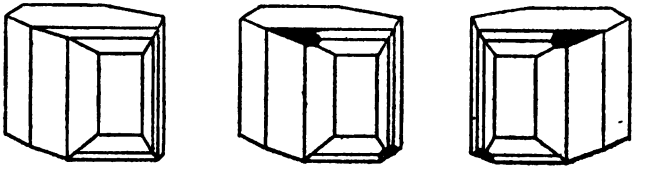

FIG. 8.

Pasteur separated each type of crystal from its neighbours and dissolved them in water. On trying the effects of the solutions upon polarized light, he found that the holohedral crystal of racemic acid was without effect, while the hemihedral forms both affected the plane of polarization. The one form turned the plane to the left, while the other form swung it exactly the same amount to the right.

Thus tartaric acid occurs in a "right-handed" and a "left-handed" form, and the mirror-image relationship of the crystals finds its counterpart in the influence of the two forms upon polarized light.

Further, when equal quantities of the right- and left-handed forms are mixed together in solution, the resulting liquid has no influence at all upon polarized light; and on

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allowing the solution to crystallize, it is observed that holohedral crystals of a new acid, racemic acid, separate out. This proves that racemic acid is really a compound of right- and left-handed tartaric acid.

Let us now look more closely into the question of an object and its mirror image. If a cube be reflected in a mirror and a new cube be made identical with the mirror image, then the cube and its mirror image will be found to be identical in every respect. If one of them could slide over the other so that the four corners of the one cube coincided with those of the other cube, then both bodies would coincide completely. But this is not the case with every body. For instance, if a person holds up his right hand in front of a mirror, the reflection is the image of a left hand; and as any one knows who has accidentally tried to put a left-hand glove on his right hand, the two hands are not superposable. Evidently, then, bodies can be divided into two classes: those which are identical with their mirror-images, and those which are not so.

When this idea is applied to molecular structure, it is clear that the same difference between two sets of substances may appear. One set of molecules may be symmetrical in their internal arrangement, so that their mirror-images will be identical with the molecules; while another set of molecules may be so constructed that their mirror-images are not superposable upon the original molecules.

This idea was put forward in 1874 by two independent workers, van 't Hoff and Le Bel, and this coincidence in ideas forms one of the most curious phenomena in science. At certain times it would appear that a new conception is, as it were, "in the air" and that specially receptive minds grasp it simultaneously and without any communication with each other. The discovery of Neptune by Adams and Le Verrier, and the publication of the idea of natural selection by Darwin and Wallace are other examples of the same kind.

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Looking back on the progress of scientific discovery, the most striking feature is not the fact that certain things have been discovered, but rather the very slightness of the barrier which so often has stood between the success and failure of a certain line of research at a given period. Again and again subjects have been approached and their problems virtually solved; yet for want of just one connecting link, or even the addition of a few words to a statement which, in itself, contains the key to the problem, the question may go unanswered for years. No better example of this is to be found than that furnished by the evolution of the conception of chemistry in space.

Pasteur, in one of his lectures, had made use of an analogy to indicate the difference between the asymmetry of the quartz crystal and the asymmetry of tartaric acid. "Consider a spiral staircase, the steps of which are cubes or other objects superposable upon their mirror images. Destroy the staircase and the asymmetry disappears. The asymmetry of the staircase was due to the method of placing together the single steps. So it is with quartz. The quartz crystal is the completed staircase. It is hemihedral, and in consequence it acts upon polarized light. If, however, the crystal is dissolved, fused, or in any other way has its physical structure destroyed, its asymmetry is no longer present, at the same time all action upon polarized light disappears. Consider, again, the same spiral staircase, the steps of which are formed by irregular tetrahedra. You may destroy the staircase, but the asymmetry remains, because you have to deal with a collection of irregular tetrahedra."

Thus the idea of a tetrahedral grouping was evolved by Pasteur, but at that date our knowledge of chemical structure was insufficient to permit him to carry his deductions further. It was left for van 't Hoff and Le Bel to bring the matter to a new stage.

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Imagine that two regular tetrahedra of the same dimensions have at each of their corners some objects which may be symbolized by the letters *a*, *b*, *c*, and *d*. An examination

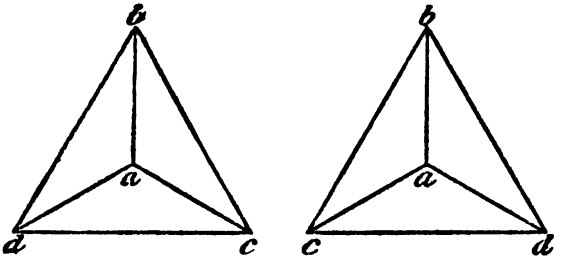

FIG. 9.

of the figure (Fig. 9) will show that two arrangements are possible, one of which is the mirror-image of the other. Now, in chemistry it is found that a carbon atom can be connected with four groups, and if these four groups are different from one another, and if the carbon atom be supposed to lie at the centre of a tetrahedron whose corners are occupied by the four groups, then clearly there will be two possible arrangements, one of which is the mirror-image of the other, just as is the case in the figure above. The two groupings will be related to one another as a right hand is related to a left hand, or as one crystal of tartaric acid is related to its antipodic form.

Le Bel and van 't Hoff suggested that those compounds which showed a power of rotating the plane of polarization probably contained a carbon atom of this type which was united to four different groups; in the case of one form the groups were arranged in one configuration, while in the isomeric compound the groups were in the mirror-image position. Such a carbon atom is termed an *asymmetric* carbon atom.

This view was speedily found to agree with the

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experimental facts, for optically active bodies were observed to contain an asymmetric carbon atom within their structure. In more recent times it has been found that other asymmetric atoms may replace the carbon atom; compounds are known in which asymmetric atoms of sulphur, selenium, tellurium, tin, nitrogen, and other elements form the foci of the optical activity of the compounds.

This conception of van 't Hoff and Le Bel brought chemistry into a new field. Previous to their investigations chemical structure had been a matter of formulæ written on paper; but with the tetrahedal conception of the carbon atom came the idea that chemical compounds must be conceived as three-dimensional groupings; so that what has been termed "Chemistry in Space," or Stereochemistry, came into existence.

Once this idea of chemical compounds as three-dimensional bodies took root, investigators began to look for other fields in which the same ideas could be utilized, and the subject soon began to ramify throughout the whole of chemistry. One example will suffice to show the general type of these developments.

If it be assumed that atoms have position, it must also be accepted that they have magnitude, and consequently they must occupy a certain amount of space. Now, imagine that two atoms are capable of uniting with one another; they will do so if they can come into sufficiently close proximity. If, however, one of them be surrounded by a number of other atoms, the entrance of the second reacting atom will become more difficult, owing to the presence of the group of atoms about the first reacting atom. These atoms may have no chemical influence upon their action, but their mere bulk suffices to prevent any interaction taking place.

It might be supposed that stereo-chemistry was one of a purely theoretical interest, but in point of fact it touches

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practical affairs at many points. One or two illustrations will serve to throw some light upon these.

It was observed by Pasteur that when a solution of equal quantities of the salts of right- and left-handed forms of tartaric acid was treated with the fungus which we know as blue mould—*Penicillium glaucum*—the fungus fed upon the right-handed compound much more readily than upon the left-handed variety. The same kind of phenomenon has been found in the case of the fermentation of certain sugars: one compound is fermented, while another, which differs from the first only in its spacial arrangement, is practically untouched. Now, the ferments used in these cases are themselves asymmetric agents, and it has been suggested by Fischer that the process of fermentation may be likened to the unlocking of a door: the ferment acts like a key, while the sugar represents the lock. The ferment "key" will fit the right-handed sugar, but will not unlock the other type. Another common analogy is furnished by the case of a right-handed bolt which will easily screw into a nut with a right-hand thread, but which will not fit a left-hand threaded nut.

Digestion is a form of fermentation in some respects, and it might be expected that a similar set of phenomena would make their appearance there also. This actually proves to be the case. Ordinary sugar can be digested with ease; but a sugar which differs from ordinary sugar only in its space arrangement does not lend itself to digestion, and passes through the system unchanged. This fact has been taken advantage of in the case of diseases like diabetes, in which it is undesirable to give the patient any sugar. The food of the patient is sweetened with the second type of sugar, which, though it has a sweet taste, is not digested like ordinary sugar, so that it has no deleterious action upon the system. In recent years, of course, this method has been superseded by the discovery of the

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substance saccharin, which, though sweet, is not a sugar at all, but has rendered the use of any sugar unnecessary.

In the action of many drugs, it is found that the spacial relations of the atoms play a great part. A derivative of the alkaloid tropine is known which has a strong mydriatic action upon the pupil of the eye; the mirror-image compound is apparently without this property. Again, cocaine exists in two forms which are called dextro and lævo cocaine;[1] the dextro body, when applied to the tongue, has a much more rapid paralyzing action than the lævo substance. In some cases the two mirror-image compounds differ in smell, and many instances are known in which the toxic properties of the two isomeric bodies are not the same. Taste is another sense which may be affected by differences in the space relations of atoms; the compound asparagine, for example, exists in a sweet form and also as an insipid variety.

These examples suffice to show that three dimensional factors come into play in the case of many of the chemical reactions connected with our vital processes. How can we explain the difference between the effect of a right-handed form and its mirror-image?

The solution of this problem is fairly simple. We have already seen that blue mould attacks one variety of tartaric acid more readily than the other, and investigations have shown that when two asymmetric substances are allowed to interact, the speed of the reaction in some cases depends upon the space arrangement of the atoms. For example, if a right-handed compound be allowed to interact with equal quantities of the right- and left-handed varieties of a second substance, a preference is shown which may result in the two right-handed compounds uniting and leaving the left-handed body almost unattacked. Everybody has felt the awkwardness of trying to shake hands

[1] Dextro- and lævo- are the terms used to designate "right-handed" and "left-handed" forms.

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with a man who puts out his left hand instead of his right, and it appears as if something analogous to this goes on even among chemicals. Now, our sensations of taste and smell probably depend upon chemical reactions; and, further, the tissues which take part in these operations are themselves composed of asymmetric substances which are capable of affecting the plane of polarized light. Thus, if we present to our asymmetric nerve-substances two compounds, one of which is right-handed while the other is a mirror-image of this, it is clear that the nerve-substance will probably react more quickly with the one than with the other; so that one compound produces a definite sensation, while the other, which does not react so readily with the nerve-substance, remains almost without action. In this way we can account for the differences in physiological action to which reference was made above.

All our tissues contain asymmetric substances in their material, and most of our nutriment in turn is asymmetrically built up. The process of assimilation depends upon the interaction of these two factors. Suppose that an animal from this world were transported into a universe which was related to our own as an object is related to its mirror-image; it would probably be found that the animal would die of starvation, although to all appearances the plants and so forth of the new world would be identical with our own. The reason for this would lie in the fact that the machinery of our animals is constructed with a view to assimilating certain forms (say the right-handed variety) of food. Transferred to this new world, the animal would find none of these forms in existence, but only the corresponding left-handed types which it could not use up.

This leads us to another problem. An examination of vital tissues of all kinds shows us that a great portion of them is built up asymmetrically. We have right-handed tartaric acid existing in plants, but left-handed tartaric

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acid is a very rare substance indeed. Experiment in the laboratory shows that these asymmetric forms cannot be synthesized directly. If a substance such as tartaric acid be prepared by purely chemical processes, it is found that the product is a mixture of right- and left-handed forms in exactly equal proportions, so that in the laboratory there is no preponderance of one form such as we find in nature. On the other hand, once we obtain an asymmetric compound in a state of purity, we can utilize it to separate the two components of an equal mixture of right- and left-handed types. For example, quinine has the power of rotating the plane of polarization, and if we make the quinine salt of synthetic tartaric acid (which is a mixture of right- and left-handed tartaric acids in equal proportions), we find that the salt of the one form of the acid is less soluble than the other form of salt; so that the two salts can be separated by a process of crystallization, the less soluble form crystallizing first from a solution of the two. On the other hand, if we crystallized the mixture of the two forms of tartaric acid from a solution, we should find them both coming out at once, as their solubilities are equal.

Thus, in order to obtain an optically active substance, we need to employ, in addition to ordinary chemical processes, a selective action. As we have seen, an enormous quantity of these asymmetric substances is found in natural products. Evidently, then, some selective process must have been at work during the history of the earth at some period or another.

Pasteur, in one of his lectures, referred to "the interest which attaches to the different optical isomers on account of this asymmetric force. It leads us to ideas concerning the secret cause which produces the asymmetric arrangement of atoms in organic substances. Whence comes this asymmetry? Why is one asymmetry produced, and not another? Go back with me to the time at which

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I recognized the absolute identity of the physical and chemical properties of the corresponding right and left rotatory bodies, but still had no idea, not a suspicion, of the difference between these bodies. It was many years later that I first recognized this difference. At that time it was to me inconceivable that nature should produce a dextro-rotatory body without its corresponding lævo-rotatory body, for the same force, the activity of which formed the dextro-rotatory tartaric acid molecule, as it seemed to me, must also produce the lævo-rotatory molecule, and thus racemic acid (a mixture of equal quantities of right- and left-handed forms) would result. But why right and left molecules? Why not only symmetrical molecules like those of inorganic substances? There are certainly causes for this remarkable behaviour of the molecular forces, even though it is difficult for us to get a clear conception of them. I believe that I am not mistaken in asserting that we now know one of its most important characteristics. Is it not necessary and also sufficient to assume that at the instant in which the plant organism arises an asymmetric force is active? For we have seen that the dextro-molecule deviates from its lævo-antipode only in those cases in which it is subjected to some kind of asymmetric action. Do such asymmetric agencies arise from the cosmic influences, light, electricity, magnetism, heat? Do they perhaps stand in close relation with the earth-movements, with the electric currents by means of which physicists explain the earth's magnetic pole? We are at present not in a position to offer the slightest suggestions with regard to these points. But I hold the existence of an asymmetric force acting at the origin of natural organic compounds as proven, while this force is lacking or is without influence upon our synthetical preparation of the same compounds, either on account of the more rapid course of the reaction or from some other unknown cause."

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At the present day chemists are divided into two schools upon this subject. The one school asserts that there is no fundamental difference between the processes of vital action and the chemical reactions of the laboratory; while the second group of investigators maintains that the question of molecular asymmetry sharply divides the two sets of phenomena from one another. Up to within a few years from the present day, neither section could produce experimental evidence in favour of the view to which it adhered. The case of sodium ammonium racemate was cited by the "uniform" school as a proof of their case. This substance, if allowed to crystallize from solution under certain conditions of temperature, separates spontaneously into two sets of crystals, which, owing to the difference in the disposition of their hemihedral facets, as we have seen above, can be separated from one another by hand. The upholders of the vitalistic theory of molecular asymmetry, however, pointed out that in order to separate the two sets of crystals from each other, vital action was necessary, since a human being was a vital agent.

The question appears to have been settled in a somewhat unexpected manner. Cotton, when examining the absorption of dextro-circularly polarized light by copper ammonium tartrate, found that the right- and left-handed tartrates did not possess identical powers of absorption. Now, if light is absorbed by any molecule, some extremely delicate rearrangement of the molecule's internal structure must take place, for it cannot be supposed that a vibration can be stopped without some expenditure of energy. Therefore, in the case of the right- and left-handed forms of copper ammonium tartrate, more energy is being used up by the one form than by the other when light is absorbed, and hence the intra-molecular readjustment is greater in the one case than in the other. In other words, dextro-circularly polarized light is an agent which favours one form of tartaric

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acid more than another. Now, Byk has pointed out that light is circularly polarized by the surface of the sea, and that one variety of circularly polarized light predominates upon the earth's surface. In this predominance we may find the asymmetric agent foreshadowed by Pasteur, and this agent, so far as the earth is concerned, is practically ubiquitous. It therefore seems not improbable that some such cause, acting under favourable conditions, may give rise to a preponderance of a right-handed or left-handed form at one particular period in geological history.

It should be noted that, once the presence of an asymmetric substance is granted, the subsequent steps up to the present complex series of naturally occurring asymmetric forms become simple to understand. A very small quantity of an asymmetric acid may, under suitable conditions, serve to decompose into antipodes a very large amount of a mixture of right- and left-handed forms without losing any of its efficacy in the process; while each portion of the newly produced products becomes a fresh centre for the decomposition of mixtures of right- and left-handed acids, which in their turn may serve to separate other mixtures into their components, and so on *ad infinitum*. The case is analogous to that of ordinary animal reproduction, since with each generation the reproductive power increases in geometrical proportion.

The foregoing sketch gives only a very fragmentary account of the advance of three-dimensional chemistry, but it will be sufficient to show the ramification of this important branch of science, and to give some conception of the manner in which a subject, at first sight of apparently little importance, may finally prove to touch upon the most important problems of existence.

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CHAPTER VIII

THE INERT GASES AND THEIR PLACE AMONG THE ELEMENTS

If a hitherto unknown substance falls under the observation of the chemist, a question at once presents itself: Is this body an element or a compound of several elements? And his first steps in the examination of the substance are generally directed towards the solution of this problem. Let us see what this implies. Suppose that the given substance is made up of several elements chemically combined together. The investigator will endeavour to split it up into its components, or, if this fails, he will try to separate one component from the rest by combining it with a new substance which will not attack the remaining portion of the compound. For example, if a given body is composed of carbon and bromine, the bromine can be separated from the carbon by allowing sodium to interact with the given substance. Sodium is a metal which has a strong affinity for bromine, but it has no affinity for carbon; so that at the end of the reaction carbon would be set free, while the bromine would combine with the sodium, yielding sodium bromide. Now, the properties of sodium bromide are well known, so that it can easily be identified; and from the fact that it is produced by the action of sodium upon the given substance, the deduction can be drawn that the latter must have contained bromine among its constituents. Again, oxygen has a strong affinity for carbon at high temperatures,

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but has little effect on bromine under the same conditions. If, therefore, the given body be heated in a stream of oxygen, and it is found that part of it is converted into a compound of oxygen and carbon (carbonic acid gas), it may be concluded that the original substance contained carbon. It was proved that it also contained bromine, so that clearly it is built up from at least two constituents. It is what we call a compound. Suppose, however, that many different methods of splitting up the substance had been tried and had failed to break it down into two or more components, we should then call it an element; and we should assume that, as far as chemical tests went, it was a homogeneous kind of matter incapable of further decomposition.

It is easy to see that, as time went on, all the commoner elements (those which occur in large quantities on the earth's surface, either alone or combined with other elements) were discovered and identified. Then came a period when new elements had to be sought for in more out-of-the-way sources; rare minerals had to be examined, and minute traces of elements were all that could be found, which increased enormously the difficulties of the researches. Finally, it appeared that all the sources had been examined in which any large quantity of a new element could be expected, and the investigations in this branch of chemistry grew more and more laborious.

Suddenly, in the last decade of the nineteenth century, a fresh source of these bodies was tapped, and within a few years a whole series of new elements was discovered. The constituents of the atmosphere had been investigated many times, but until twenty years ago a really thorough search in this region had been hampered, partly by the lack of suitable methods and also by the preconceived idea that the earlier investigators had been sufficiently thorough in their work to preclude the possibility of any further discoveries of importance being made. No one had thought that there

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was any likelihood of finding unknown gases in the air; and yet, there, awaiting their discovery, lay five elements, not only new, but with properties so strange as to differentiate them sharply from all the elements ever examined before. Not only so, but their detection involved a recasting of our ideas on the classification of the elementary substances, although the scheme then current had stood the test of a generation's work. Before proceeding further, it will be well to gain some idea of the method employed in what is called the Periodic Arrangement of the elements.

One of the most characteristic properties of a body is its weight. Suppose that the weights of two substances have to be compared; it is easy to put each of them into the pan of a balance and find which weighs more. But when greater accuracy is required, it is necessary to measure them both against some standard, in order to find out the exact difference in weight between them. Now, if the difference in weight between two letters was to be ascertained, no one would think of using a ten-pound weight to test them; it would be better to choose a unit weighing less than either letter. The same rule holds good in weighing the elements; a weight is chosen which will enable us to differentiate easily between them. Hydrogen is the lightest of all the elements, and it has therefore been selected as the standard with which all the rest are measured, so that the unit adopted has been the weight of a hydrogen atom.[1]

[1] In recent years it has been found more convenient to take a unit based upon the oxygen standard. When the weight of the oxygen atom is taken as 16·00, it is found that a large number of elements have atomic weights more closely approximating to round numbers than is the case when the weight of the hydrogen atom is taken as unity, so that chemical calculations are thus simplified. Thus with hydrogen = 1·00 we have boron = 10·9, carbon = 11·91, columbium = 93·3, gallium = 69·5, manganese = 54·6, and mercury = 198·5; whereas, taking the oxygen standard, these figures become integers, 11, 12, 94, 70, 55, and 200. Thus in chemical arithmetic we in this way get rid of the fractions, which simplifies calculation.

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In this way it is ensured that no result is less than unity. Reckoning the weight of the hydrogen atom as one, the weights of other elements are represented by a series of numbers which are termed atomic weights. Thus the chlorine atom is 35·5 times heavier than the hydrogen atom, so we say that the atomic weight of chlorine is 35·5; similarly, nitrogen atoms are about fourteen times heavier than hydrogen ones, so we take 14 as the atomic weight of nitrogen. The atom of sodium is twenty-three times heavier than a hydrogen atom, whence it follows that sodium's atomic weight is 23.

Having got the atomic weights of the various elements, the next step is to arrange them in ascending order, placing hydrogen first, and when this has been done, it is found that certain curious relations come to light which are not obvious at the first glance. The work of Döbereiner, Newlands, Lothar Meyer, and Mendeléef brought these inter-relations into a systematic form; and at the present day the chemist is able to draw up a table which brings into clear relief the curious periodicity in the properties of the various elements known to us. The complete table is shown on the sheet at the end of this volume, but here it may be as well to give a few indications of the relations which it brings into prominence.

Suppose that we begin with a few of the lighter elements, and write their names and approximate atomic weights in a line:

Lithium	Beryllium	Boron	Carbon	Nitrogen	Oxygen	Fluorine
7	9	11	12	14	16	19

The next element in order in Mendeléef's time was sodium, with an atomic weight 23; but this substance is closely allied to lithium in all its properties both physical and chemical, and it in no way resembles fluorine. For instance, both lithium and sodium are white metals of low specific gravity, easily cut with a knife; both tarnish when

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exposed to air, and both react readily with acids. On the other hand, fluorine is a gas, does not displace hydrogen from acids as do sodium and lithium, but interacts readily with hydrogen itself, in a way that the metals cannot do. It is clear that in any logical arrangement of the elements, we must bring out the close resemblance between sodium and lithium, and the marked difference between them and fluorine. How can we do this? Let us begin a new line and write sodium under lithium.

The next element in order of increasing atomic weight is magnesium, and just as sodium is found to resemble lithium, so does magnesium prove akin to beryllium. Next we come to aluminium, which is found to approximate in its properties to boron. Then comes silicon, which manifests a kinship with carbon in many respects. Continuing in this way, we get our second line of elements:

Lithium	Beryllium	Boron	Carbon	Nitrogen	Oxygen	Fluorine
7	9	11	12	14	16	19
Sodium	Magnesium	Aluminium	Silicon	Phosphorus	Sulphur	Chlorine
23	24	27	28	31	32	35·5

When the whole of the elements known in Mendeléef's time were arranged in this way, it was found that they fell naturally into their places, but that a certain number of gaps were left in the table. These gaps represented elements still to be discovered at that date. So convinced was Mendeléef of the correctness of his arrangement, that he permitted himself to prophesy the properties of these still unknown bodies, and these prophecies form one of the most striking achievements in the branch of speculative chemistry. Let us see how he was able to accomplish this.

In the Periodic Table at the end of this volume, the seventh column contains a group of elements which will serve to illustrate the point fairly well. These bodies,

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fluorine, chlorine, bromine, and iodine, are known for convenience as the halogen group. Now let us examine their physical and chemical properties and see if we can trace any regularities in them.

In the first place, take the physical properties. At ordinary temperatures, fluorine is a light gas, chlorine is a heavy gas, bromine is a heavy liquid, while iodine is a solid. Evidently, then, the density of the elements increases as we go down the column, each member being heavier than the one above it. Next, the colours of the substances show a similar relation to each other. Fluorine is a pale greenish-yellow body, chlorine is a much deeper greenish-yellow, bromine vapour is red, and the vapour of iodine is deep violet in tint. Thus, as we pass down the column, the colour of the bodies deepens. Then in the boiling-points of these elements there is a repetition of the same variation. Fluorine boils at $-187°$ C., chlorine at $-35°$ C., bromine at 59° C., and the boiling-point of iodine is 184° C. Similarly, the melting-points differ in the same way; fluorine melts at about $-223°$ C., chlorine at $-102°$ C., bromine at $-7°$ C., and iodine at 114° C. Clearly, from element to element down the column, a steady change takes place in all the physical properties—an increase in density, a deepening in tint, a rise in the melting-point and boiling-point.

Turn now to the chemical properties. Take the case of a compound made up of one atom of sodium and one atom of iodine. When this is brought into the presence of bromine the latter element drives out the iodine, attaching itself to the sodium in its place. Evidently the bromine has a stronger affinity for sodium than iodine has. On bringing the compound of sodium and bromine obtained in this way into contact with chlorine, the next element in the series, the result is quite analogous to the one we obtained before. The bromine is driven out and the chlorine attaches itself to the sodium. So that clearly the chlorine atom has a

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greater affinity for sodium than the bromine atom has. If the experiment be repeated, using fluorine as the one reagent and the compound of sodium and chlorine as the other, it would be found that the fluorine displaced the chlorine, and in its turn combined with the sodium. This series of experiments makes it quite evident that the chemical activity of the halogens varies regularly throughout the series; fluorine is the most active, then comes chlorine, next there is bromine, and finally iodine is the weakest of all.

Now let us imagine that we were not acquainted with the properties of bromine, and were asked to predict them merely from its position in the Periodic System. We should draw up a table somewhat as follows:—

Fluorine.	Light gas. Light greenish-yellow. Boils at −187° C. Melts at −223° C. Displaces chlorine from its compounds.
Chlorine.	Heavy gas. Dark greenish-yellow. Boils at −35° C. Melts at −102° C. Displaces iodine from its compounds.
Bromine.	?
Iodine.	Solid. Gives violet vapour. Boils at 184° C. Melts at 114° C.

We should then attempt to find some properties intermediate between those of chlorine and iodine; and these, after checking them by a general comparison with those of fluorine, we should ascribe to bromine. Let us see what this leads us to. Chlorine is a heavy gas, while iodine is a solid. The intermediate state evidently would be the liquid one. Then chlorine is deep yellowish-green in tint, while iodine is violet. The intermediate colour is red. Again, fluorine boils 152° lower than chlorine, so we should expect bromine to boil about the same amount lower than iodine. This would give a boiling-point in the neighbourhood of 32° C. (actually it is 59° C.). Then fluorine melts 121° C.

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lower than chlorine, so that by analogy we should expect bromine to melt about 120° lower than iodine. This gives the figure $-7°$ C., which is the actual melting-point of bromine found in practice. Further, since fluorine displaces chlorine from its compounds, and chlorine in its turn displaces iodine, we should anticipate that the element intermediate between chlorine and iodine would displace iodine, but would be displaced in its turn by the more reactive chlorine. From what has already been said with regard to the real properties of bromine, these deductions agree fairly closely with the facts. It is clear, therefore, that the Periodic Table can be utilized to predict the properties of elements simply by comparing the properties of the missing element's neighbours.

It might be argued, however, that the results would not be so good if we were really ignorant of the properties of the element we were discussing, for if we know them already we may be biased. The best reply to that is to give side by side the predictions made by Mendeléef and the properties of the elements which were eventually discovered some years after he had published his forecasts. One example will suffice, and we may choose the element germanium as an illustration. Mendeléef published his prediction in 1881, while the element itself was not known until 1886.

Prediction.	*Properties of Germanium.*
Atomic weight = 72.	Atomic weight = 72·5.
Specific gravity 5·5.	Specific gravity 5·47.
Element will be dirty-grey.	Germanium is greyish-white.
Oxidation will convert the element into an oxide containing one atom of the element and two atoms of oxygen.	Germanium behaves in this way.
The oxide will be white.	Germanium oxide is white.
Acids will have a slight action on the element; alkalies will have no pronounced action.	Germanium is not attacked by hydrochloric acid. It is acted on by a mixture of concentrated nitric and hydrochloric acids. It is not acted on by a solution of caustic potash, but can be attacked by molten potash.

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Prediction.	*Properties of Germanium.*
The chloride of the element will be a liquid boiling under 100° C., and having a specific gravity of 1·9 at 0° C. It will contain one atom of the element and four atoms of chlorine.	Germanium chloride boils at 87° C., and at 18° C. has a specific gravity 1·887. It contains one atom of germanium and four of chlorine.
The fluoride will not be gaseous.	Germanium fluoride is a white solid.
The element can be prepared by acting on a double fluoride of the element and potassium with metallic sodium.	Germanium is prepared in this manner.

The foregoing will be sufficient to show that Mendeléef's arrangement of the elements was evidently based upon very sound foundations, and for a quarter of a century it remained in its main outlines practically without alteration. Here and there a new element was discovered and found to correspond with a blank space in the original table; but in 1893 the table contained the same general grouping as in 1869, and no one could then have foreseen that in less than a decade a new series of elements would be discovered the recognition of which would entail the addition of a complete new column to the table, while their properties would be such as to have defeated any attempt to foretell them by analogy with any of the other elementary bodies.

The discovery of even the first of these new elements would have been a great achievement in itself; but when it is recalled that on the chemical side our knowledge of the whole series is associated with the name of the leader of a single school of investigators, it is hardly too much to say that the case is without parallel in the history of chemistry. When the record of the discovery of these substances is told in full, it will form one of the most fascinating chapters in the history of science; for the present, however, we must content ourselves with a bare outline of the story.

The first hint of the presence of these elements in the atmosphere was given by a research of Cavendish in 1785; but it was not until a century after this, when Cavendish's

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experiment had been entirely forgotten, that the subject was again taken up. Cavendish, in the course of his investigations, had tried the effect of passing electric sparks through air contained in a tube. He absorbed the resulting oxides of nitrogen, as well as carbon dioxide, by means of alkali; and any residual oxygen was removed by the aid of liver of sulphur. After long-continued sparking, he obtained a very small bubble of some gas which could not be got rid of like the others. Some idea of the saving of labour resulting from modern methods and apparatus may be got when it is realized that what can now be accomplished by the mere switching on of an induction coil was only to be achieved in Cavendish's day by the continuous grinding of a frictional machine by hand, and that in the experiment which we have described Cavendish and his assistant had to spend three weeks in turning the machine's handle before they obtained the final result. The quantity of substance produced in this way was too small to permit any further experiments to be made on its properties, and it was only after argon had been isolated in some considerable amount that Cavendish's work was again called to mind.

More than a century after Cavendish's experiments, namely, in 1894, the subject was revived by some work of Lord Rayleigh's. At that time he was engaged in the accurate determination of the densities of various gases, and in the course of this work he had to weigh a certain volume of nitrogen. In all chemical work it is desirable to apply as many controls as possible, and one series of checks is applied by the examination of samples of the same substance which have been derived from different sources or have been prepared by different methods. This decreases the possibility of an undetected impurity running through a series of experiments; for when different sources of material are utilized, it is very improbable that exactly the same quantity of impurity will occur in each of the

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various samples. Thus, if the results obtained do not concord among themselves, it is usually possible for the investigator to lay his finger on the faulty sample, and then take steps to eliminate the impurity from future specimens.

Rayleigh, on these grounds, resolved to extract nitrogen from the atmosphere, and check the results thus obtained by making a second series of weighings in which chemically prepared nitrogen was used. The results in the two cases were not concordant, for a litre of nitrogen from the air was found to be 0·0064 gramme heavier than a litre of nitrogen which had been prepared by chemical process, as the following figures show:—

One litre of atmospheric nitrogen weighed	1·2571 grammes
One litre of chemically prepared nitrogen weighed	1·2507 „

Now, some cases are known in which an element occurs in two or more forms which are called allotropic modifications. Thus phosphorus is found to exist as yellow phosphorus or red phosphorus; various different kinds of sulphur are known, each of which has its own peculiar physical attributes. Rayleigh was therefore inclined to assume that his results could best be explained by supposing nitrogen to be capable of existing in two allotropic forms, and that the one form was present in different proportions in air and in chemically prepared nitrogen, so that the densities of the two different mixtures would not be the same.

On the other hand, Sir William Ramsay, when the results of Lord Rayleigh's experiments were published, suggested that the difference in density between the two samples of nitrogen was more simply explicable on the assumption that Rayleigh's "atmospheric nitrogen" contained a small quantity of a heavier gas which was not present in the nitrogen prepared by chemical means. To settle the question, the two investigators joined forces and

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began an examination of the nitrogen which is obtained when the commoner constituents of air are removed.

We may describe Ramsay's method first. In the course of his lecture experiments he had occasion to burn magnesium in a limited supply of air; and he observed that the resulting substance, when treated with water, evolved ammonia gas, which is a compound of nitrogen and hydrogen. This suggested to Ramsay that, by heating magnesium in "atmospheric nitrogen," it would be possible to convert all the nitrogen into a compound of magnesium and nitrogen, and thus leave behind the unaffected heavier gas which he supposed existed in "nitrogen" from the air. The method was a very simple one. All that was necessary was to pass some "atmospheric nitrogen" over heated magnesium in a tube; collect the unaltered gas and pass it over more heated magnesium, and so on until all the nitrogen had been combined with magnesium. The gas was weighed after each passage across the magnesium, and in this way an idea was obtained as to the progress of the experiment.

It was found that the density of the gas gradually increased as more nitrogen was withdrawn by the action of the magnesium. Now, on the hydrogen standard, nitrogen has a density of about 14, while oxygen has a density of 16. Oxygen was a possible, though not very probable, impurity in "atmospheric nitrogen"; so that as long as the density of the residual gas remained below 16 there was no proof that a new element had been found. With repeated passages across the heated metal, however, the density of the residual gas slowly increased; it passed 16, and when at last it rose to 17·5, it became clear that a new gaseous element had been discovered. Finally, the density of the remaining fraction of gas touched 19, and there it remained constant, the pure substance having at last been obtained.

By this process no less than a hundred cubic centimetres

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(about six cubic inches) of the new gas had been obtained, and an examination of its spectrum showed that in the green and red regions it exhibited lines which could not be identified with any lines characteristic of the elements discovered up to that time.

In the mean time, Rayleigh had been at work independently; and by employing Cavendish's method of passing sparks through a mixture of oxygen and "atmospheric nitrogen" he was able, with the modern apparatus at his disposal, to isolate about a quarter of a cubic centimetre of the same gas. By means of diffusion experiments he showed that the new gas was present in greatest quantity in the less diffusible portions of air, so that it evidently was a comparatively dense substance. The rate of diffusion of any gas is inversely proportional to the square root of the density of the gas, whence it follows that a heavy gas will diffuse more slowly than a light gas.

Further investigations of the properties of the new gas showed the collaborators that it was actually an elementary body; but the details of these researches are too technical to enter upon in this place.

Now, most elements will enter into combination with either magnesium or oxygen at high temperatures, but Ramsay's experiments had shown that the new gas would not combine with magnesium, while the work of Rayleigh proved that the same was true of oxygen. It was therefore evident that the new element was singularly inert in its chemical behaviour, and on this ground it was christened by its discoverers argon (from ἀργόν, idle). Its atomic weight was found to be approximately 40.

The next clue in this chain of researches came from a wholly unexpected quarter. Hillebrand had been engaged in an examination of certain minerals containing uranium, and he had observed that, when treated with acids, these bodies evolved a gas which had a spectrum very similar to

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that of nitrogen. Not being very certain whether the difference was due to observational error or not, Hillebrand described the gas as nitrogen. Ramsay's work on atmospheric nitrogen had naturally made him anxious to obtain this gas from fresh sources, for it seemed possible that other new elements might be associated with nitrogen as argon was in air. He therefore procured some specimens of cleveite—one of the minerals in question—and found that on boiling it with sulphuric acid he obtained a gas whose spectrum showed a brilliant yellow line. This line lay close to the position occupied by the yellow line in the spectrum of the metal sodium, but careful comparison showed that the two lines were not identical. When the spectrum of the new gas was brought into the field of a comparison spectroscope along with the sodium spectrum, there was a slight difference in the position of the two yellow lines, but it was so small that at first Ramsay was inclined to believe that his instrument was at fault.[1] He therefore took his spectroscope to pieces and rebuilt it, in order to avoid any possibility of error; but still the two lines refused to coincide. It thus became clear that a second new gaseous element had been discovered.

The question now arose: Had this body ever been noticed by earlier workers, though not carefully examined by them, just as was the case with regard to Cavendish's work on argon? It seemed improbable that it had actually been isolated; but there appeared to be a possibility that it might have been recognized as appearing among those substances which had been detected in the stars by spectrum analysis, though they had not been isolated on the earth's

[1] An inspection of the frontispiece will show how slight is the difference in position between the two lines. Both occur near the point 585 on the scale. The helium spectrum shown is of some interest, as the specimen whose spectrum is reproduced was one of the earliest obtained by Sir William Ramsay, and was presented by him to Prof. Letts, who kindly lent it to the author for photographic purposes.

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surface. Ramsay therefore submitted specimens of the gas to Sir William Crookes, who mapped the body's spectrum and found that the characteristic yellow line was coincident with a line called D_3 in the spectrum of the solar chromosphere. This line D_3 had first been observed by Janssen in 1868, and Sir Norman Lockyer had attributed it to an element which he christened helium (from ἥλιος) on account of its occurrence in the sun. Thus by Ramsay's work, helium, which had up to that time been only a "hypothetical" element, was shown to exist among the earth's constituents.

An investigation of the properties of helium showed that it belonged to the same chemical class as argon. Like its predecessor, it was a chemically inert, monatomic gas; and an examination proved that its density was only twice that of hydrogen. It was, therefore, the second lightest element known.

Another question then arose. What place had these new bodies in the Periodic Classification of the elements? Examination of the table showed that no position could be found for them within the limits of the original arrangement, and it became evident that a new column would have to be added to the table in order to include them in the classification. This was done; and helium was placed in a new column next to lithium, while argon was fitted into position to the left of potassium. The following section of the table will show the result; the numbers under each name give the approximate atomic weights of the corresponding elements:—

Helium	Lithium	Beryllium
4	7	9
(—)	Sodium	Magnesium
20	23	24
Argon	Potassium	Calcium
40	39	40

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But, as will be noticed from the above, this left a gap between argon and helium which should have been occupied by some element with an atomic weight of approximately 20. Its position is marked by the sign (——).

A very laborious search for the missing element was undertaken by Ramsay and Travers. All sorts of expedients were tried, and every likely substance was investigated. They examined seven meteorites, a score of waters from mineral springs, and about a hundred and fifty minerals, but without success. Chemical methods were also applied. The magnesium compound obtained from atmospheric nitrogen was tested to see if it contained anything beyond ordinary nitrogen; but here also the search failed; and it seemed as if the quest was to be fruitless.

At this time, however, a new line of investigation was opening up which was destined to play its part in the history of the inert elements. Liquid air became much more readily obtainable, owing to a simplification in the mode of its production.

The principle on which the liquefaction of air depends is very simple. If a gas be heated it expands; or, to put the same thing in another way, if a gas be caused to expand, heat is absorbed in the course of the expansion. Now, suppose that a gas is compressed to a very high pressure and that it is allowed to expand suddenly. It must absorb heat from some neighbouring body; and if the expansion be sufficiently rapid, it will be unable to obtain enough heat from surrounding objects quickly enough. Yet the heat must come from somewhere, or the gas could not expand. What happens is this. Part of the gas expands rapidly; in so doing it absorbs heat from the remaining portion of the gas, and this latter part becomes much cooler than it was at first. If this cold portion of the gas be utilized to cool down the compressed gas which is being let into the apparatus, it is clear that

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the second liberation of gas will result in a further cooling effect, and that this second cooling will produce a lower temperature than before. The cooling effects are thus cumulative, so that by continually liberating the gas from a high-pressure reservoir and cooling each section before it is liberated, the temperature can finally be lowered until the gas is liquefied. The following diagram will show the principle of the apparatus employed (Fig. 10).

Ramsay and his assistants, having the Hampson liquefier at their disposal, turned their attention to the gases of the atmosphere, in the hope that, in addition to argon, there might be further undiscovered elements in the air. A small quantity of liquid air was allowed to boil away until very little remained. This residue was then freed from oxygen and nitrogen in the usual manner, and when its spectrum was examined it was found that two new bright lines were present, one in the green, the other in the yellow region. The density of the gas was next determined, and in this impure specimen it was found to be about 22·5; since argon has a density of only 20, it was clear that this body could not be argon, but must be some heavier substance. The new substance, after being purified and examined carefully, was found to

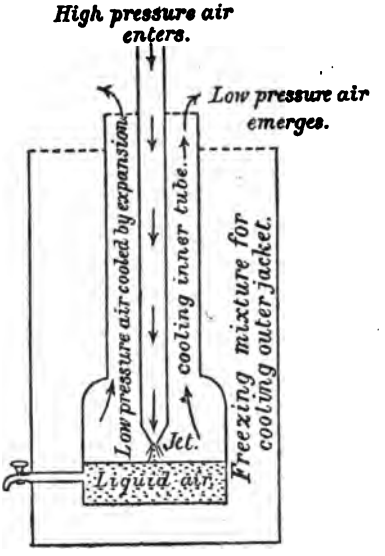

FIG. 10.

Diagrammatic representation of the principle of gas liquefaction apparatus.

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be a third new element, to which the name krypton (κρυπτόν = hidden) was given.

Some crude argon, obtained from the atmosphere, was next liquefied by the aid of liquid air; and this experiment resulted in the discovery of a fourth new gaseous element, which has been called neon (νέον = new). The specimens of neon obtained in this way, however, were found to be contaminated with helium, which had also proved to be present in the atmosphere; and it became necessary to devise some means of separating the one element from the other. Liquid air did not supply the necessary low temperature, and Ramsay cast about for some method of obtaining still colder bodies.

At that time, liquid hydrogen had been obtained by Olszewski; and this substance seemed likely to be suitable as a means of separating neon from helium; for much lower temperatures can be obtained with liquid hydrogen than it is possible to reach with liquid air. Unfortunately, Olszewski's machine was only capable of producing traces of liquid hydrogen. Sir James Dewar had devised a method of obtaining the liquid in quantity, but as he has retained the secret of his process, his method was not available to Ramsay.

A new apparatus, planned by Travers, was built in the laboratories of University College, London, and upon its first trial, a few months later, this machine produced no less than a hundred cubic centimetres of liquid hydrogen.

A mixture of impure neon and helium from the air was now passed into a flask cooled with liquid hydrogen; and it was found that the neon froze, while the helium remained as a gas. The latter was pumped off, and the solid neon was gradually warmed and then purified. An examination of its density showed that it has an atomic weight of twenty.

When these experiments were over, a fresh examination

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of the residues of liquid air was made. A long and very laborious process of fractional distillation was carried out, with the result that the krypton was separated from the argon. The latter boils at a lower temperature than the former; so that a separation of the two could be carried out by allowing the frozen mixture gradually to grow warm and then pumping off the argon, leaving the still frozen krypton behind. Of course, this process had to be repeated many times before all the argon was removed. When the remaining krypton was allowed to become warm, it was found that some gas, apparently not krypton, remained in the flask. This was collected, and an examination of its spectrum showed that yet another new element, xenon (ξένον = strange), had been discovered. This substance was shown to have the atomic weight 128.

These new elements were now fitted into their places in the Periodic Table, and the result can be seen from the section given below.

Helium	Lithium	Beryllium
4	7	9
Neon	Sodium	Magnesium
20	23	24
Argon	Potassium	Calcium
40	39	40
Krypton	Rubidium	Strontium
84	85	87
Xenon	Cæsium	Barium
130	133	137
(Niton)	——	(Radium)

At that time, the two elements niton and radium were undiscovered. Their history belongs to other chapters in this volume.

It might be thought that there are possibly other

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unknown elements of this group present in the atmosphere, but if this be the case they must exist in almost inconceivably small quantities. Moore has fractionated the residues of no less than a hundred and twenty tons of liquid air which were furnished to Sir William Ramsay by the Société Air Liquide; but he found no trace of any gas other than those already known. The methods employed by him were so delicate that he could have detected at least 0·03 of a cubic centimetre (one five-hundredth of a cubic inch) of any new gas; so that if any new element did exist in the original hundred and twenty tons of liquid air there cannot have been more than one part of it present in 2,560,000,000 parts of air. It seems improbable, after this, that any further addition will be made to the list of the inert constituents of the atmosphere.

The following figures give a rough idea of the very minute proportions in which the various inert elements are found in the air:—

Helium . .	4 parts in every 1,000,000 parts of air.
Neon . . .	1 to 2 parts in 100,000 parts of air.
Argon . .	0·937 part in 100 parts of air.
Krypton . .	1 part in 1,000,000 parts of air.
Xenon . .	1 part in 20,000,000 parts of air.

We must now turn to the properties of these elements. It will be best to give a short description of the general characteristics of the series, and then mention one or two details in which each member differs from the others.

All five elements are gases at ordinary temperatures, as has already been seen in the account of their discovery. They are entirely different from any other elements known, in that they appear to be absolutely inert as far as their chemical relations are concerned. They may be mixed with the most violently reactive elements, such as fluorine, which attacks almost every other element; and yet no

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reaction occurs between the two gases. They can be heated to a very high temperature in contact with metals, but even then no compounds of them are formed. In fact, it has been impossible to obtain compounds of them with other elements. Their place in the Periodic Table forms rather an interesting feature of the classification. If we write down the series of elements after helium, we get the following group :—

Helium	Lithium	Beryllium	Boron	Carbon	Nitrogen	Oxygen	Fluorine
4	7	9	11	12	14	16	19

Now, lithium is what is termed an electro-positive element ; beryllium is less markedly electro-positive ; carbon, standing midway in the series, is neutral ; while the rest of the group increase in electro-negative character as we go nearer to the right-hand side of the table, until, on reaching fluorine, we come to an element which is as strongly electro-negative as lithium is electro-positive. The next element in the series is neon, which is absolutely inert ; and it shows no trace of chemical properties whatsoever, either electro-positive or electro-negative. After this comes the strongly electro-positive sodium. Thus at the end of one line we have a very markedly electro-negative element, fluorine, and this is separated from the next element (which is electro-positive) by this inert element neon. Apparently the inert elements form a kind of bridge in the table between two sets of elements of opposite electrical properties.

In connection with the table another point may be mentioned, which has perhaps struck the reader. It will be remembered that in drawing up the table we arranged the elements in order of increasing atomic weight. Now, taking the elements having atomic weights approximating to 40, we find that chlorine is 35·46, potassium is 39·1, argon is 39·88, and calcium is 40·07. It is clear that on a logical basis argon should lie in the table between potassium and calcium. But this would bring it out of a position among

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the inert gases and throw it in with the alkali metals ; while potassium would be placed among the helium group instead of argon. Such an arrangement would obviously be worthless ; and numerous attempts have been made to determine whether the atomic weights of potassium and argon could not be shown to be slightly different from the figures we have just given. It appears, however, that these are the true atomic weights, as far as our present experimental resources can define them ; so that argon and potassium form an anomalous pair of elements with regard to the Periodic arrangement. There is one other similar pair of elements known ; for the relations of iodine and tellurium are exactly parallel to those of argon and potassium. These two anomalies have caused much speculation, but we are at present without any valid explanation of them.

Let us now go over the various elements of the inert gas series, and see what points of interest arise in connection with each of them. We may begin with helium. This, as has already been mentioned, is the second lightest element known, being only twice as heavy as hydrogen. It is a colourless, tasteless gas, and has the lowest refractive power of all the gaseous elements. In addition to occurring in the solar photosphere, it appears to be present in bodies emitted from terrestrial volcanoes, for its characteristic line, D_3, has been observed in the spectra of some eruption products of Mount Vesuvius. As we have seen, it is also found in some minerals, such as cleveite, bröggerite, and fergusonite ; and some mineral springs have been found to contain quite a high percentage of helium. It is of some interest to note that, while the lighter gas hydrogen can be liquefied with some difficulty, helium for a considerable period defeated all attempts to convert it into the liquid form. In 1908 it was liquefied by Onnes, and it was then found that its boiling-point lies in the neighbourhood of $-268°$ C. Every other gas has been solidified, even hydrogen having yielded when

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the liquid is allowed rapidly to evaporate *in vacuo;* but so far helium has resisted all attempts to prepare it in the solid form.

Turning now to the next member of the series, neon, we find that its special peculiarity is connected with its spectrum. When excited by an electric discharge in a vacuum tube, helium glows with a yellow light, which is due to the D_3 line. Neon, on the other hand, shines with a tinge between rose and orange, and displays one of the most beautiful tints of all the gases. This luminescence is remarkable for the ease with which it can be excited. For example, the Hertzian waves, which are employed in wireless telegraphy, are produced when an electric spark is allowed to pass through air; and if these waves are allowed to sweep across a vacuum tube containing neon at a low pressure, the gas in the tube glows under the influence of the wave. This property has been utilized in order to measure the wave-length of the Hertzian vibrations. The sensitiveness of neon to electricity has further been demonstrated by Professor Collie. A tube containing neon at a low pressure along with a few drops of mercury will show a brilliant glow of orange light when the tube is tilted so as to allow the mercury to flow along the side. It is supposed that the friction of the mercury globules against the sides of the tube generates a very slight amount of electricity, and that under the influence of this very minute charge the gas glows with its characteristic spectrum. If a quartz tube be substituted for one of glass, the mercury can be boiled without cracking the vessel; and in this case a beautiful greenish-white illumination appears, extending throughout the upper part of the tube.

With regard to the other elements, little can be said. They are all odourless, tasteless gases, like neon and helium. Argon's spectrum is red, but much duller than that of neon; krypton shows a splendid pale violet light when an electric

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discharge is passed through it at low pressures; while xenon emits a sky-blue luminescence.

We have now completed our survey of these inert gases of the atmosphere. Alike in their absence of chemical properties and their extraordinary scarcity, they differ from all the other elements with which we are acquainted, and until the discovery of argon, no one could have foreseen that substances such as these could exist. They fill the gap in the Periodic Classification between two series of elements of strongly marked opposite characters, and thus they give to the table an effect of balance which it lacked before their presence on the earth had been detected.

In conclusion, it should be pointed out that though in the foregoing description the experiments which led to their discovery appear to be simple, yet in actual practice the technique employed was necessarily of a much higher order than can be appreciated except by those who have actually worked in this branch of the subject. The handling of small quantities of gases is a difficult art, and it was carried to a high pitch in these investigations of the inert atmosphere. Yet, as will be seen in a later chapter,[1] this work on argon and its congeners was destined to be surpassed in another portion of the same field when Ramsay and his collaborators came to deal with the last element of the group, niton.

[1] See Chapter X.

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CHAPTER IX

RADIUM

IT is almost a generation since the first steps were taken in the long series of investigations which have culminated in our present-day knowledge of radioactive phenomena, and when we look back at the various stages through which the subject has passed, we must be struck by the unexpected developments which have led on from one point of vantage to another until a series of disconnected fields has been joined together and our views of the constitution of the elements and of matter have been altered and clarified. From its earliest beginnings, radioactivity has been a common ground upon which chemists and physicist have met; and in the progress of the subject we find the physicist perforce becoming a chemist, while the chemist undertakes the most delicate physical operations. A subject so full of unwonted spectacles is well worth tracing to its source.

In 1879, Sir William Crookes directed his attention to the study of electric discharges in high vacua. When two wires are inserted into the ends of a glass tube a current from an induction coil is passed through the wires; then, if the coil be sufficiently powerful, a spark crosses the gap between the two wires. Now if the ends of the tube be closed and a side-tube be inserted in the vessel thus formed, it is possible, by means of a pump, to reduce the air pressure within the tube to any required extent. Let us imagine that this is being done while the electrodes are

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kept in connection with the working induction coil. At first the usual spark discharge will occur; but as the air pressure is lowered, it becomes more difficult for the spark to pass across the gap between the electrodes. Soon the sparks cease and are replaced by a glow in the tube. When the pressure sinks to about a three-thousandth of an atmosphere, the discharge produces in the tube a mass of luminous striæ. Finally, as Crookes was the first to show, the striated appearance vanishes when very high vacua are used; and the whole tube becomes practically non-luminescent in the interior. But at this point a new phenomenon makes its appearance: near the negative electrode, the glass of the tube itself begins to glow with a bright apple-green phosphorescence.

Further investigations threw more light upon these results. If, instead of a wire, a metallic plate be used as the negative electrode, it is found that rays appear to start from it, travelling in lines perpendicular to the surface at which they originate; and it is where these rays strike the glass of the tube that the green light is produced. If there be interposed in the path of the rays a metallic object, such as an aluminium cross, we find that a shadow of this is thrown on the green patch.

Owing to the fact that a tiny windmill revolves when placed in the path of the "rays," Crookes took the view that these "rays" were really made up of solid particles which were projected with enormous velocity from the charged surface of the electrode, and he supposed that these particles were the residual molecules of the gas which originally filled the tube. Since the properties of these hypothetical residual molecules differed entirely from those of ordinary gases, he concluded that in the Crookes tube we have a new state of matter, "radiant matter" in addition to the solid, liquid, and gaseous type with which we commonly deal.

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The nature of this "radiant matter" became the next problem demanding a solution. It might be, as Crookes suggested, merely the residual molecules of gas in the tube; or, on the other hand, it was possible that the particles were really tiny fragments of the metal of the electrode which were torn off by the disruptive force of the discharge. To decide between these two possibilities was fairly simple. A tube was constructed in which the electrode was made of platinum, and the discharge was allowed to pass through it for long periods. If any platinum had been torn off the electrode, it would be carried across the tube and splashed against the glass at the point where the luminosity was visible; but a careful examination of this spot even after prolonged experiments failed to reveal any trace of platinum deposit. Taken in conjunction with the fact that platinum electrodes do produce a deposit in their immediate neighbourhood (which shows the possibility of detecting such projected particles by that method), this experiment of Crookes proved that the exciting cause of the green light at the end of the tube could not be a bombardment of the glass by flying metallic fragments.

We are thus thrown back upon other hypotheses to explain the nature of the "cathode rays" as they are called. They are not particles detached from the cathode; but they might be gas molecules as Crookes suggested, or they might be actual vibrations in the ether like light-waves. Both these explanations, however, failed to stand the test of further investigation. It was shown by Lenard that if the cathode rays be allowed to strike an aluminium window instead of the glass of the Crookes tube, they pass through the metal and emerge into the air. But in the air, they become scattered and weakened to such an extent that a thickness of four inches of air suffices to extinguish the ray. Carrying this line of research further, it was found that the stopping power of a medium was independent

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of its chemical nature, but was directly related to two factors: the density of the screen used and the thickness of it. This evidence, which is supported by other data, points clearly to the rays being composed of material particles of very small size which are intercepted in their path by the molecules of the screen. The thicker the screen, the more molecules a projectile will encounter on its way through; while the denser the screen is, the more tightly packed will be the molecules composing it, and hence a greater number will lie in the same thickness of material.

Again, if two independent rays be separately focussed upon the same point of glass in a tube and the two jets are then brought into play at once, it is found that the paths of the two rays diverge from their original lines and become averted from each other to some extent. This tends to show that the rays are really streams of similarly electrically charged particles; for such particles repel one another, and hence two streams of them would exercise a mutually repulsive influence.

By this reasoning we come to conceive the cathode rays as a stream of electrically charged particles, and the final problem before us is to determine the mass of the particles in the stream. This cannot be done directly, but by a fairly simple method we can estimate the velocity of the particles and the ratio between their masses and the electric charges which they carry. An analogy will make the technique of this easily comprehensible. Suppose that we have a billiard ball travelling in a straight line up the centre of the table. If we push it gently with our finger at right angles to its path, we shall deflect it to the left, say, so that it will strike the top cushion halfway between the centre and the left-hand top pocket. If we gave it a harder push we should deflect it still more, so that it would run into the left-hand top pocket. Thus the amount

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of deflection of the ball from the original path will depend upon the force with which we push it. Now a lighter ball would be more easily deflected than a heavy one; so that if we knew the amount of force exerted by our finger, and were able to measure the amount of deflection from the straight, we should be able to make a guess at the weight of the ball we were using.

Replace the ball by the cathode particle travelling with high velocity in a straight line; instead of a finger, imagine that magnetic and electric forces of known magnitude are being employed, and we then have a picture of the essential features of the method which is used to measure the ratio between the mass and charge of the particle. The reason that we cannot by this method get a measure of the mass alone will be evident on consideration. Suppose that we are dealing with the action of an electrical force upon the charged particle. Evidently, if this force be kept constant, and the particle has a certain charge, the repulsion between the one electrical charge and the other will be the force deflecting the mass of the particle from its straight path. Now suppose that we had a particle with double the charge upon it and that this particle had double the mass of the first one, it is evident that while we have doubled our repulsive force we have also doubled the amount of mass which that force has to act upon, so that the deflection in this case would be the same as the first. Thus, all we can deduce from the experiment, if we get two equal deflections, is that the ratio of charge to mass is the same for each of the two particles we are using.

Now let us see what results have been obtained in this way. In the first place, we must get some idea of the electrical charge which an atom of, say, hydrogen can carry. When hydrochloric acid, a molecule of which contains one atom of hydrogen combined with one atom of chlorine, is dissolved in water, the molecule is disrupted

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into two portions, and the hydrogen portion is capable of taking up a charge of electricity. If the two poles of a battery be dipped into the solution, it has been shown that a process is going on which might be likened to the action of a ferry-boat; the hydrogen atom has on board a certain charge of electricity which it ferries across to one of the poles of the battery. This is, of course, a very crude simile, but it will serve the purpose. Now, just as a ferry-boat has a certain maximum load of passengers, so the hydrogen atom can only carry a certain charge of electricity. This charge can be measured by means into which it is not necessary to enter here; it is sufficient to know that it is a constant quantity. Now, from the measurements of the ratio of mass to charge in the case of the cathode particles, it has been found that if these were hydrogen atoms they would be carrying a charge a thousand times greater than that which a hydrogen atom in solution carries with it. This would be an enormous charge. If we imagine, however, that the mass of the particles is much less than that of a hydrogen atom, the charge would also be reduced in the same ratio; and this brings us to more probable figures. Taking the charge on the cathode particle as being of the same order of magnitude as that on a hydrogen atom, then the mass of the particle would be about a thousand times less than that of a hydrogen atom. But such a small mass of matter is much more minute than anything we know; so the view gradually gained ground that these cathode particles were not matter at all, in the accepted sense, but were actually minute corpuscles of electricity. This view has been supported by an enormous amount of experimental data; and it is generally accepted that the cathode rays are streams of particles of electricity—electrons, as they are called nowadays. Thus we have come to regard electricity, not as a kind of property of matter, but as

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actually something which can have an existence of its own apart from matter as we know it.

We must now turn to another set of phenomena which were discovered in connection with these highly evacuated Crookes tubes. In 1895, Röntgen observed that the green fluorescent patch of glass where the cathode rays struck the tube was the centre of origin of a new set of rays which are now termed the Röntgen or X-rays. Every one is acquainted with the main properties of these new rays: their invisibility to the eye, their power of passing through many solid substances, their action on the photographic plate, and their power of causing certain substances like barium platinocyanide to glow. The exact nature of the Röntgen rays is at present unknown, but various hypotheses have been put forward to account for them. On one view, the X-rays are really pulses in the ether like light-waves, but differing from the latter in an important respect. Light-vibrations are rhythmical, like a musical note, while the X-rays are supposed to be irregular vibrations caused by the impact of the cathode-ray electrons upon the glass of the tube. We could liken the light-waves to the sound-waves from a tuning-fork, while the X-rays would be similar to the noise caused by throwing a handful of small shot against a tin can. Another hypothesis suggests that the X-rays, like the cathode rays which give rise to them, are really corpuscular in nature; they are streams of tiny projectiles travelling out from the glass with tremendous velocity. However, we need not enter here into the details of these suggested explanations.

The foregoing paragraphs may at first sight appear to have little connection with radium; but, as will be seen hereafter, they are in reality so closely related to the phenomena of radioactivity that it has been necessary to describe them here in order to avoid subsequent reference

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to the same subjects in places where it would break the chain of the narrative.

It will be recalled that the spot where the cathode rays strike the wall of a vacuum tube is marked by a green luminescence, and that the X-rays originate from this shining patch on the tube. Now, certain minerals shine with a phosphorescent light which, to the naked eye, resembles the green light of the Crookes tube very closely. A year after the discovery of the X-rays by Röntgen, Becquerel began an investigation of the latter phenomenon; for it seemed possible that the phosphorescence of the minerals might be a sign that radiations similar to the X-rays were being given off by those substances. Following up this line of thought, he discovered that when some crystals of potassium uranium sulphate were placed upon a sensitive photographic plate which was wrapped up in black paper, he obtained marks upon the plate similar to those produced by an X-ray discharge. This experiment led to the discovery of what are now known as the Becquerel rays.

The resemblance between the Becquerel rays and the X-rays proved to be very close. Neither type of rays can be subjected to refraction by a prism; and in this respect they both differ from ordinary light, which is refrangible. Both types of ray can pass through solid bodies. Again, the X-rays have the power of affecting gases through which they pass in such a way as to make the gases more ready to conduct electricity; and the Becquerel rays also have this power of "ionizing" gases, as it is termed scientifically. A concrete example may be given here, as the experiment we shall describe is one which is often used in practice in order to determine whether a substance is giving off Becquerel rays to any considerable extent.

If the edges of two small slips of gold leaf be gummed to

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the opposite sides of a piece of metal suspended in a vertical position, the parts of the gold leaf slips which hang downwards from the metal will lie parallel to each other under normal conditions. If, however, both slips of gold leaf are charged with positive electricity, they will at once exert a repelling action upon each other and will tend to fly apart as far as possible. Being retained by their edges to the metallic slip, they bend outward at their free ends, just as the leaves of a book fall apart when it is opened. An instrument of this kind is termed an electroscope. Now, if an electroscope be charged so that its leaves spring apart, and if it be kept in a well-insulated case, a considerable period will elapse before the leakage of electricity through the air allows the leaves to fall together again. This is due to the fact that air in its normal condition is a bad conductor of electricity. The length of time which is required to bring the leaves together in dry air can be easily measured. If the electroscope be recharged again and a substance which is giving off Becquerel rays is brought into the neighbourhood of the leaves, then it is found that the leaves of the electroscope fall together very much more rapidly than they did in the previous experiment. The collapse of the leaves is due to the fact that the air, which previously conducted electricity very badly, has now become a good conductor, so that the charges leak away from the leaves much more quickly than before, and hence the leaves soon lose their repelling power upon each other. It is obvious that this method may be capable of quantitative use. If two substances are tested, one of which sends out great quantities of Becquerel rays while the other is but weakly emittent, it is clear that the first substance will cause a greater leak of electricity than the latter, and consequently the leaves of the electroscope will fall together much more rapidly in presence of the first substance than with the second. Hence the time taken for the electroscope leaves

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to lose their charge and fall together is a measure of the activity of the two substances as far as the production of Becquerel rays is concerned. This period is the actual one employed to test the radioactivity of substances and compare them with each other.

We have seen that salts of uranium were found to emit these Becquerel rays, and further investigation proved that the metal uranium (from which the salts can be formed by the action of acids) also had the power of radiation. Evidently, the origin of the rays is to be sought in the metal; and the next question which suggested itself was whether or not similar elements might not also have the same property. This line of research led to the discovery of radium itself.

Uranium compounds are found in nature in much the same situations as the compounds of the rare earth elements of which we have spoken earlier in this volume. Pitchblende, a mineral which is usually associated with the igneous rocks, is the chief source of the uranium derivatives, and soon after the publication of Becquerel's results, Madame Curie began an investigation of the various minerals of this type. The results at first sight appeared inexplicable, for it was found that in some cases the natural ore was much more radioactive than the amount of uranium which it contained would be if it were liberated from the impurities. For example, some pitchblendes are about four times as active as metallic uranium; and a mineral called chalcolite (which is a uranium derivative) was found to be twice as active as the amount of uranium in it would lead us to expect. Finally, Madame Curie prepared artificial chalcolite in the laboratory from pure metallic uranium, and when tested, this artificial chalcolite was found to be much less radioactive than the naturally occurring mineral. How can these results be explained?

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The solution of the problem is not difficult. Suppose that the natural products contain a substance, some kind of impurity, which is not present in the artificial mineral, and assume that this impurity is much more radioactive than even metallic uranium. Then obviously the natural bodies will have a higher radioactive power than the synthetic ones.

Following this line of thought, Madame Curie began a long and tedious process of analysis with the object of isolating the highly radioactive impurity from the pitch-blende. Into the actual details we cannot go, but the process can be understood if we assume that acid can dissolve certain parts of the mineral, while other chemicals present remain insoluble in the acid. The solution and the solid left behind are then separately tested by means of the electroscope, and in this way it may be found, for example, that the chief radioactive power is left to the solid, while the substances which have gone into solution do not show any great sign of activity. The solid is then treated with some other reagent which removes some further constituent in solution, and the test with the electroscope is repeated. In this case it may be found that the radioactive substance has gone into solution, and an attempt is then made to separate the components of the solution from each other by a further chemical operation. In this way, after many fruitless efforts, a salt was obtained which we now know as bromide of radium.

Radium bromide is a white substance when freshly prepared, but after a time it gradually becomes coloured with a tint ranging from yellow to rose-pink. A freshly recrystallized specimen of the solid substance glows with a pale blue luminescence, but this property is weakened with the passage of time. When dissolved in water, radium bromide decomposes the latter with the evolution of oxygen and hydrogen; so that the phenomenon is

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similar to the effect produced when two poles of an electric battery are immersed in water.

Probably the most striking characteristic of radium salts, however, is their property of continuously emitting heat. It was found that the temperature of a radium salt is always slightly higher than that of the surrounding air; and apparently this evolution of heat goes on steadily and spontaneously. It has been estimated that pure radium would evolve in an hour a quantity of heat sufficient to melt a piece of ice rather heavier than the radium used. This phenomenon in itself placed radium in a unique position among the substances known to chemistry. The whole of our scientific ideas rest upon two main foundations: the conservation of mass and the conservation of energy. The first of these dogmas lays down that though the properties of matter may be altered, matter itself cannot be destroyed; after being subjected to any process whatever, its mass will remain the same as at the start. The conservation of energy implies much the same idea. We may begin with a given quantity of heat, transform it into light, convert that into chemical energy, then into electricity, then into mechanical energy, and finally change this last into the original form of heat, and we shall find that in the process there has been no loss of energy for which we cannot account. All the energy has either reappeared as heat or has gone in overcoming the friction of our machinery; it has not been annihilated. But if this be true, the converse must also be correct; if we cannot destroy energy, neither can we create it. Yet here, in radium, we have a substance which apparently gives out a steady supply of heat without undergoing any visible change. We shall see later on where this heat comes from; but at the time when the phenomenon was first observed the element radium appeared to be endowed with properties of a most mysterious type.

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Metallic radium has been obtained in very small quantity by Madame Curie. It is a brilliantly white metal which rapidly tarnishes in air. When thrown into water it decomposes the latter violently, with the evolution of hydrogen and oxygen; and it blackens paper with which it is left in contact.

Before dealing with the most interesting properties of radium, which centre round the rays which it emits, we may devote a paragraph to the other radioactive elements which have been discovered in recent years, for their properties are closely allied to those of radium in this field. Since the beginning of the radium researches, quite a number of elements have been discovered which would probably have remained unknown for many years had it not been for the electroscopic methods of investigation introduced into chemistry at the beginning of the study of radiochemistry. Uranium was, of course, the first radioactive element known. In addition to it we are now acquainted with polonium, actinium, radio-lead, and ionium, all of which show marked radioactive powers. Thorium, also, is strongly radioactive; and the incandescent gas mantle, whose development we traced in a previous chapter, has brought the possibility of at least one experiment in radioactivity within the reach of every one. It will be recalled that the ordinary Welsbach mantle contains a very large proportion of thorium oxide; and as thorium is a radioactive mineral, each mantle is capable of emitting a certain amount of the Becquerel rays. If one of these mantles be left in close contact with a photographic plate for some hours, the plate, when developed, will show on its film an imprint of the reticulations of the mantle, due to the rays emitted from the thorium compound impressing the sensitive silver salts.

Let us next take up the question of the Becquerel rays, and see how the nature of these has been determined.

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In attacking this problem, we are faced with several possible solutions, and we had better take these in order. First, the Becquerel rays may be vibrations of the ether, like light-waves; secondly, they may be akin to the X-rays, for there are striking resemblances between the two types of radiation, as we have already seen; thirdly, the Becquerel rays may be streams of material particles shot out with high velocity; fourthly, the stream hypothesis may be correct, but the particles may be electrons, and not fragments of matter; and, finally, the Becquerel rays may be a mixture of two or more of the foregoing, *e.g.* they may be partly ethereal vibrations and partly streams of material particles.

How are we to decide among all these hypotheses? Two methods suggest themselves at once. In the first place, we might interpose in the path of the rays some substance which would act as a kind of ray-filter, allowing some portion of the rays to pass through it, while the remainder of the rays was stopped by the medium; or, secondly, we might direct the rays through a magnetic field, and observe whether the rays as a whole were diverted from their path or whether there was a selective divergence, some parts of the ray being less affected than others. Both of these methods have been found to yield results which prove that the Becquerel rays are not homogeneous, but must be considered as being made up of various different types of ray. Let us take the first method, and describe an experiment made by Strutt which shows this very clearly.

Suppose that an electroscope is charged so that its leaves diverge from each other, and that under normal conditions the leaves take an hour or more to fall together, owing to the leakage of electricity through the air. Now imagine that a small portion of a radium salt is brought near the electroscope; then, owing to the passage of the

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Becquerel rays, the air becomes a much better conductor of electricity, and, consequently, the leakage increases to such an extent that the leaves fall together in, say, ten seconds. Now take the case where the radium salt is wrapped up in a piece of tinfoil and replaced where it was in the previous experiment; it is now found that the leaves do not fall together so rapidly, for it may take, say, a hundred seconds for them to collapse. This proves that the rays from the wrapped-up radium salt are not so active as those emitted by the unprotected salt; and the logical deduction is that some of the rays have been intercepted by the tinfoil, while another section of them has not so been obstructed. This, however, does not end the results obtainable in this way. The interposition of one thickness of tinfoil sufficed to reduce the rays to one-tenth of their power, and, on the same reasoning, two thicknesses of foil should reduce them ten times further, so that the electroscope would then be discharged only after a thousand seconds. That is what would occur if all the rays were of a uniform nature. The experimental result shows, however, that the interposition of the second sheet of tinfoil hardly makes any change in the time of collapse of the electroscope leaves, which still amounts to about a hundred seconds. This proves conclusively that in the Becquerel rays we are dealing with two different radiations, one of which is stopped by tinfoil, while the other is not affected by an increase in the thickness of the sheet of foil. When the same process is carried further, sheet lead being used instead of tinfoil, the results are analogous; one set of rays is stopped by increasing the thickness of lead foil, while another set appears to be uninfluenced by such an increase. Thus it is possible to filter the Becquerel rays into three groups: first, the rays which cannot penetrate tinfoil; second, those which can pierce tinfoil, but cannot pass through sheet lead; and, lastly, those rays which can

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traverse considerable thicknesses of sheet lead without any diminution in their powers.

This method of filtering, however, throws no great light upon the nature of the various components of the Becquerel rays; it merely tells us that they are separable into three

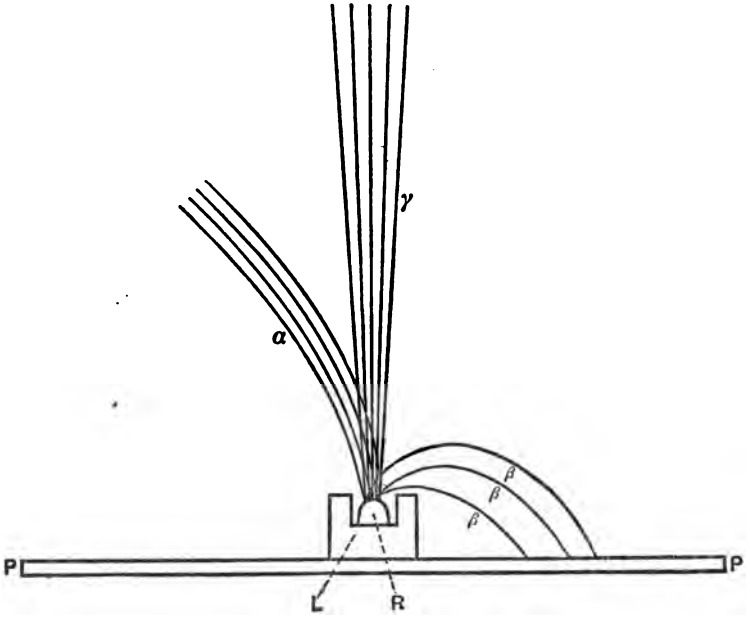

FIG. 11.

classes. The application of a magnetic field to the rays yields much more satisfactory results.

In Fig. 11 we have a diagram which can be easily understood. L is a piece of lead in which a hollow has been drilled, and in this hollow is placed some radium salt R. A magnetic field is supposed to be applied at right angles to the plane of the paper, and directed toward the paper. Under the influence of this field, we find that the

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Becquerel rays split up into a triple sheaf, each of the three components having properties different from those of the others. These three types of radiations are termed α-, β-, and γ-rays. The γ-rays, as can be seen from the figure, are not deflected by the magnetic field, and in this they resemble ordinary X-rays. They differ from the X-rays, however, in having a much greater penetrative power. The β-rays apparently resemble the streams of particles which jet out from the cathode of a high vacuum Crookes tube; for, like these, they are diverted by the magnetic field, as can be seen from the figure. It seems probable that they are streams of electrons, or particles of negative electricity. The deflection which their paths undergo when subjected to the magnetic field is very considerable; and if a photographic plate be laid under the leaden cell in the position indicated by PP, the spots where the rays touch can be detected by the marks which they leave upon it. The remaining components of the Becquerel rays, the α-rays, are deflected also by the magnetic field, but their line of deflection is opposite to that of the β-rays. From this, since the β-rays are negatively charged, we deduce that the α-rays are positively electrified; and it seems probable that they are electrically charged atoms of helium moving with high velocity.

We may now take up in turn the properties of these three constituents of the Becquerel rays, so far as they are known. Let us begin with the γ-rays. As we have mentioned, these rays are not deflected from their original paths even by the most powerful magnetic fields at our disposal. Their power of penetration is very high, for Strutt has found them capable of passing through a sheet of lead three inches in thickness. They appear to have all the properties of the ordinary X-rays in an enhanced degree, so that it seems probable that they are similar to the X-rays in their nature.

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With regard to the β-rays, there seems to be no doubt as to their character. They are particles shot out from radium, just as cathode particles are ejected from the electrode of a vacuum tube; but the velocity with which they emerge is far greater than that of the cathode particle. It is probable that these particles are the most swiftly moving bodies with which we are acquainted, and some idea of their tremendous velocity may be gathered from the following figures:—

Velocity of light per second	190,000 miles.
„ „ β-rays „	170,000 to 40,000 „
„ „ cathode rays	60,000 to 15,000 „

The α-rays, which close our survey, are much slower in their motion; the stream of positively charged particles of which they are composed moves at a speed of approximately 20,000 miles per second. Thus a body travelling with the velocity of the α-rays could go round the earth's equator in a little over a second, while in the same time a β-particle would cross two-thirds of the gulf between the earth and the moon.

The nature of the α-rays throws some light upon the curious property of radium which we mentioned above, viz. its peculiar power of continuously emitting heat. It is well known that, when a rifle bullet strikes a wall, heat is generated by the impact. Now, if α-particles are liberated in the interior of a radium salt they will strike against the surrounding material almost as soon as they are free to move, and these collisions account for the store of heat which radium is unintermittently giving out. These clashings of the particles are by no means mere figments of scientific imagination; for Crookes has devised an instrument called the spinthariscope which can be bought for a few shillings, and by means of which the actual impacts can be observed with the naked eye. The principle upon

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which it depends can be grasped from the following experiment, which can be made by any one. Take a couple of lumps of loaf-sugar into a dark room and rub them together briskly. If they are carefully watched, small flashes of light will be seen, which are due to the fracture of the crystals of sugar. This phenomenon is termed triboluminescence. Now, instead of a crystal of loaf-sugar a crystal of zinc sulphide might be broken, and the flash of light would again make its appearance. If a layer of zinc sulphide be placed just below a trace of some radium salt which is glued to a tiny pointer, the α-particles shot out by the radium will strike the crystals of zinc sulphide at such a high velocity as to shatter them, and each collapse of a tiny crystal will produce a flash of light. In order to make things clearer, the spinthariscope is furnished with a magnifying glass focussed on the zinc sulphide layer, and on looking through the lens intermittent flashes of light like shooting stars are visible, each of which represents the impact of an α-particle upon the sulphide screen.

In the foregoing paragraphs we have taken the properties of each type of ray separately; but it must be remembered that radium's action is due, not to one or other type of ray, but is the sum of all the united effects of the α-, β-, and γ-rays. Since many experiments on the radioactive power of radium salts have been carried out by simply bringing radium into the vicinity of the body to be acted upon, our knowledge in such cases is confined to the general influence exerted by the Becquerel radiations. We may mention one or two of the more striking results which have been obtained.

It will be recalled that the X-rays have the power of lighting up a fluorescent screen coated with barium platinocyanide. The Becquerel rays also are able to do this; but they are capable, in addition, of causing glowings from other substances which are not influenced to any

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extent by an X-ray discharge. For example, paper, cotton, and such-like bodies show a luminous appearance in presence of radium salts; a diamond, which shows no luminescent power with the X-rays, can be made to shine by placing radium near it. In some cases, this power of glowing in presence of radium is lost by the material after a prolonged exposure to the Becquerel rays; but it can usually be recovered again if the substance be exposed to light for a time. Heat has the same effect in the case of some glasses which, owing to long exposure to the radiations, have lost their original power of fluorescing.

Among the properties of radium, however, one stands out above the others in its interest for the general public. It was observed by an experimenter in the early days of radium investigation that when unprotected radium salts were carried about in the pocket of his waistcoat, a small ulcer was formed in the flesh directly under the spot where the radium had rested. This demonstrated that the Becquerel rays could produce very deep-seated alterations in the vital tissues, and recent work has shown that these changes in the tissue take a considerable period to mature. We may handle a tube of radium continuously for a fairly prolonged period without noticing any ill effects, we may then discontinue working with it for some weeks, yet at the end of this time a sore may develop in the flesh which has been submitted to the action of the radium. Thus the investigation of radioactive substances is a very troublesome line of research, for any injuries which may be sustained take a long time to show themselves, and in the mean time the investigator may be quite unconscious that any harm has been done. One of the most serious dangers is due to the fact that the Becquerel rays have the power of killing spermatozoa; so that, unless proper precautions are employed, sterility may result.

This is the dark side of the shield; but we must not

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forget that the applications of radium in medical practice make up the credit side of the account. It has been shown that in certain cases of ulceration, the submission of the wound to the action of the Becquerel rays appears to have a beneficial effect, and in this direction we are only at the beginning of what promises to be a most fruitful field of inquiry. Already radium is coming into the hands of medical specialists, and when its powers are fully understood, there seems to be no doubt that it will be of immense importance in the alleviation of disease.

We have now gained some insight into the properties of radium, and it may be of interest to take up the problem of the origin of this mysterious body. Before we can do that, however, we must glance at another question. In connection with radioactivity in general, the most striking feature is the amount of energy which radioactive substances liberate spontaneously. We know that radium is steadily emitting heat, light, and Becquerel rays, and we are forced to ask whence comes this energy which is continually being dissipated. There are two possible answers to such an inquiry. In the first place, radium may be regarded as a kind of transformer, a machine which takes up energy from the outside, changes it into another form of energy, and then liberates it again. On this view, radium would be like a steam engine which absorbs energy in the form of heat and liberates it as mechanical power. This is a physical suggestion. On the chemical side, another view suggests itself. Radium may be a very unstable body which is continually decomposing, and this decomposition may be accompanied by a liberation of energy, just as the firing of a dynamite cartridge sets free a considerable store of energy in the form of mechanical power. Recent work has shown that the chemical view is the correct one; so radium must be considered to be an unstable substance which is slowly disintegrating with the

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accompaniment of a great setting free of energy in one form or another.

The evidence upon which this view is based is very simple, and a summary of it is not without interest. It will be recalled that uranium is a radioactive substance. Crookes, in the course of an examination of this element, found that by certain chemical methods he was able to separate pure uranium from another substance which he found in his sample of active uranium. This latter body he termed uranium-X. Examination now showed that all the activity of the original sample was concentrated in the uranium-X, while the pure uranium had ceased to be radioactive. At first there seemed to be nothing of theoretical importance in this point; for it could be explained on the assumption that uranium when pure is not radioactive, and that any activity it shows is due to an admixture of some active uranium-X. After a time, however, Crookes had occasion to re-examine his "inactive" uranium, and to his astonishment he found that it had again become active. The same observation was made by Becquerel, who had separated uranium from uranium-X by another method. We are thus faced with the indisputable fact that a sample of uranium, after apparently being freed from all radioactive products, will, on standing for a time, re-acquire radioactive properties.

There is only one plausible explanation of this phenomenon: uranium itself must be undergoing some change which gives rise to the radioactive uranium-X. Thus, when all the uranium-X is removed from a sample of active uranium and the "inactive" uranium is set aside, it at once begins to decompose and to liberate from within itself more uranium-X, which is markedly radioactive.

It might at first, by analogy, be assumed that this decomposition of uranium was a chemical reaction, but there is weighty evidence against this idea. All known

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chemical reactions are influenced by temperature to a greater or less extent, and it is usually found that a rise of a few degrees makes a great difference in the speed at which a reaction progresses. For example, the velocity with which the carbon and hydrogen of a taper combine with oxygen is very slow; but if the temperature of the taper be raised by heat, it combines readily with oxygen —in other words, it catches fire. Yet the transformation of uranium into uranium-X goes on steadily at the same rate whether the temperature be that of a winter's day or a white heat. Evidently, this breakdown of uranium into uranium-X is not like any chemical reaction we know. Again, in chemical reactions, we are dealing with the interaction of one atom with another; but this does not seem to be the case with radium. For example, if we took 226 grammes of radium and combined them with 71 grammes of chlorine, we should find that this 297 grammes of radium chloride had exactly the same radioactive power as was shown by 226 grammes of radium combined with 160 grammes of bromine to make 386 grammes of radium bromide. It is clear that the only factor in the case is the quantity of radium in the salt; the nature and weight of the other elements have no influence upon the radioactive power. Hence we deduce that the radium atom is the origin of the radioactive phenomena; and this again differentiates radioactivity from the ordinary chemical reaction, in which at least two atoms must take part.

This evidence, then, leads us to one conclusion and one only. Since the radium atom is the focus of the phenomena of radioactivity, it is in the radium atom that we must seek for the source of the energy which is so lavishly scattered in heat and the Becquerel rays. And such a liberation of energy implies that some vast change is taking place at the centre of the disturbance; so that we are driven to conclude that in the atoms of radioactive elements decompositions

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are taking place which are accompanied by the freeing of what, on a tangible scale, would be almost inconceivable stores of energy.

Rutherford has based on this idea an hypothesis which at the present time is regarded as the best explanation of the various phenomena exhibited by radioactive bodies. The Disintegration Hypothesis, as it is called, can be understood by means of a crude simile. Suppose that a radioactive atom be symbolized by a kind of Roman candle, which has just been lit. The Roman candle is now in an unstable condition; it must burn away. In burning, it throws out a star, which would represent the ejection of an α-particle by the radioactive atom. But in neither case can the process stop here. The radioactive atom has been altered into a new kind of atom, which in its turn is unstable; it therefore throws off a new α-particle, just as the Roman candle shoots out a second star, and so on. Thus we might start, say, with an atom of thorium. This, being unstable, breaks down, giving a new atom with different properties, while at the same time an α-particle is thrown off. The new atom, called thorium-1, being unstable in its turn, changes into an atom of thorium-2, which has different properties. A further decomposition results in the formation of thorium-3, which, by the liberation of another α-particle, passes into a body termed thorium-X.

Now, the stages in this series of decomposition are not by any means merely imaginary. Many of them have been predicted by means of the electroscope; and when chemical tests were employed it was found that the new bodies postulated actually did exist, and differed chemically from the substances from which they originated. It must be borne in mind, however, that in many cases these changes are slow in the extreme. Were this not so, our stock of radium would be dwindling in our hands almost

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as fast as we could isolate it. Let us try to gain some idea of the duration of these processes.

Imagine that we wished to present a gramme of radium to the nation to be kept in perpetuity, and let us see what the fate of it would be. If it were examined thirteen and a half centuries hence, it would be found that it had diminished to half a gramme; thirteen and a half centuries later the half-gramme would have lost half its weight, so that the custodian in the year 4600 A.D. would find that he had only a quarter of a gramme in his keeping. When another thirteen and a half centuries had elapsed, this quarter-gramme in its turn would have lost half its weight, so that only one-eighth of our original gift would remain undecomposed.

So much for the future; but now apply the same reasoning to the past. What would be the previous history of our gramme of radium? Obviously, since it has lost half its weight in thirteen and a half centuries, it must have weighed two grammes in the year 564 A.D. Then another step back of the same length brings us to 786 B.C., when its weight would have been four grammes; about B.C. 3500 it would be about 16 grammes in weight; while in the neighbourhood of B.C. 6000 it would weigh about sixty-four grammes. Now even this long retreat back into the past does not take us out of what we may call historical times, for we are acquainted with the history of Egypt even at this period; beyond that stretches the prehistoric era; then we come to the earlier stages of the Quaternary period; and behind them lie countless centuries before we reach the earlier days of the earth's existence. Now, calculation shows us that if we go back only a trifling matter of 140,000 years, our gramme of radium will have had as its parent a mass of radium as great as the whole earth at the present day; but we know from geological deposits that the earth has had a much longer history than this, and that during

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that history it was not by any means composed entirely of radium. Evidently there is something erroneous in our assumptions about radium.

The weak point is easy to find. We have been assuming that our present radium supply is the residuum of a much larger store of radium which once existed; but, as we have seen above, active uranium-X is not an independent substance, but is a degradation product of uranium. Why should radium not be the disintegration product of some other material? The researches of Boltwood, Rutherford, and Hahn have shown that this is the correct explanation of the apparent anomaly, and they have traced the parentage of radium until we finally reach uranium. The stages in the life-history of radium and its kindred substances are shown in the table on opposite page, the term "half-life period" indicating the length of time which elapses before a substance loses half its weight by atomic disintegration.

It is true that in this way we are merely throwing the question back a stage; for though the immediate origin of radium is cleared up, yet we are no nearer to the ultimate question of the genesis of the radioactive elements. But by tracing the line of descent of radium we at least free ourselves from the anomalous position shown in the preceding paragraphs. If it be taken as proved that radium is being regularly produced from uranium at the present day, then our supply of radium does not represent the final stage of decay of a vast quantity of radium salts, but is really quite a recently produced material: and thus it is not necessary to assume that in the earlier stages of the earth's history radium was very much more plentiful than it is nowadays. If the degradation of uranium is a very slow process when compared with the rapid decay of radium, the quantity of radium in the earth's crust at any given time need never be large; and it might even

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come about that at times the supply would practically be zero.

Chain of descent.	Half-life period of substance.	Nature of rays emitted.
Uranium	—	α
↓		
Uranium—X	22 days	β and γ
↓		
Ionium	—	—
↓		
Radium	1350 years	
↓		
Niton	3·8 days	α
↓		
Radium—A	3 minutes	α
↓		
Radium—B	21 minutes	No rays
↓		
Radium—C	28 minutes	α, β and γ
↓		
Radium—D	About 40 years	No rays
↓		
Radium—E_1	6 days	No rays
↓		
Radium—E_2	5 days	β and γ
↓		
Radium—F	143 days	α
↓		
?	—	—

With regard to the place of the radioactive bodies among the elements, a glance at the Periodic Table at the end of this volume will prove instructive. At the very foot of the Table, in Group VI., lies uranium, with the largest known atomic weight of all. Next to it there is a space left for some element with an atomic weight of about 235; this body may yet prove to be one of the decomposition products of uranium, for up to the present the quantities of these bodies obtained have been so small as to hinder any

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exact investigation of their chemical nature. The next element in order of atomic weight is thorium, in Group IV. Then there is another unknown body; and finally comes radium, which is obviously allied in chemical nature to barium, strontium, and calcium. It is clear that the atoms of radium, thorium and uranium are very heavy; thus the uranium atom is four times heavier than an atom of iron, and twenty times more weighty than a carbon atom. One might be tempted to suggest that the intra-atomic structure of these atoms was extremely complex, and that it was therefore more liable to get out of joint and break down than is the case with the simpler and lighter atoms: but this is the purest hypothesis. We do know that the heaviest atoms are the most radioactive; but beyond this we cannot do more than make plausible suggestions with regard to the cause of radioactive phenomena.

Before leaving the Periodic Table, attention may be drawn to another element which is very closely connected with radium, as we shall see in the next chapter. In Group O of the Table, at the foot of the column, lies the substance niton; and as we have seen in the scheme of the uranium degradation products, this element is one of the results of the decomposition of radium. The fact that niton is placed in the zero group of the Table indicates that it is allied chemically to the inert gases as well; so that we have thus got a bridge between these peculiar bodies on the one hand and radium on the other. As will be seen in the next chapter, this relationship is even closer than it appears at first sight.

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CHAPTER X

NITON

We have seen in the previous chapter that the radioactive elements are in an unstable condition, and that this instability leads to their decomposition into simpler substances. The present chapter will be devoted to tracing the course of the researches which led to the isolation of one of the most important degradation products of radium; and the properties of this body, niton, will be found to be even stranger than those of radium itself.

The first observation which gave a clue in this line of investigation was made by Owens in 1899. He was then engaged in examining the radiations of thorium compounds, and in the course of his work he found that his results were not so constant as, according to anticipation, they should have been. On going more closely into the matter, he was able to track down the source of the variations. When he carried out his experiments in closed vessels, he found no great deviations; but when draughts passed over the unenclosed apparatus, he found that errors crept in. These results pointed to the fact that the radioactive effects are due, in part, to some substance which can be blown away by a current of air: and we might suppose that this material was either a gas or else a cloud of very fine particles which had been detached from the main radioactive mass.

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This line of investigation was carried further by Rutherford, who placed it beyond doubt that something in the nature of a cloud did actually emanate from thorium; and he was able to show, finally, that this material was gaseous, and was not simply a mist of fine thorium particles. The emanation could be blown about by currents of gas; it can be bubbled through liquids without being absorbed, as a collection of dust-particles would be; and a mica stopper is sufficient to retain it in a vessel. The final proof of its gaseous condition was very ingenious. It has been known for a considerable time that the ordinary City fog is caused by the condensation of droplets of water round the dust-particles of the air; each fragment of dust forms a condensation centre for a drop of water, and these tiny drops form a mist when they are sufficiently numerous. This phenomenon has been applied by Wilson to estimate the number of dust-particles in air. It is found that when filtered air is used, a lowering of the temperature does not necessarily produce condensation of the water-vapour which the air contains; but if particles of any description are floating in the air, then condensation can take place under the same conditions. We cannot count the number of dust-particles easily, as they are so tiny; but we can count the number of drops of water much more readily, and as each drop of water represents one dust-particle, it is possible to form some estimate of the number of dust-particles present if we count the number of water droplets produced when the temperature of the air in a given space is lowered to the condensing point. Wilson's dust-counter, as it is called, depends upon this principle. Now, when the emanation from thorium was let into the dust-counter, it was found that no condensation of water-vapour occurred such as would have made its appearance at once had there been any solid particles of thorium floating in the air; and as

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this is one of the most refined methods of detecting floating particles, it seems clear that the thorium emanation is really a gaseous body.

The rate of decay of this emanation from thorium is extremely rapid; in less than a minute it loses half its radioactive power. It might be thought that such a rapid degradation could not be accurately measured, but a very ingenious method was employed which enabled the half-life period to be ascertained. Pure air was blown across a thorium salt, and in its passage it took up some of the emanation from the thorium. The mixture of air and emanation was then passed into a long brass cylinder through whose wall projected three insulated electrodes. By this means it was possible to measure the activity of the emanation as it passed each electrode, and knowing the rate at which the gas was being passed along the cylinder it was easy to ascertain the rate at which the emanation was losing its radioactive power.

These investigations on thorium suggested that other radioactive bodies might also be liberating emanations; and an examination of actinium and radium showed that this was actually the case. The emanation from actinium is very short lived, as it loses half its activity in less than four seconds. Radium, on the other hand, gives off the gas niton, whose half-life period is between three and four days. As the properties of niton have been investigated more fully than those of the other emanations, we may confine our attention to it; but it should be borne in mind that as far as they are known, the other emanations resemble it very closely.

Before entering into a description of the properties which have been found to characterize niton, attention must be drawn to the very great experimental difficulties of the researches in this branch of the subject. The maximum amount of niton which could be obtained from one gramme

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of radium under ordinary conditions is 0·6 of a cubic millimetre. No more than this volume can be obtained at a given time from the gramme of radium, for, as will be seen later, the niton decomposes very rapidly as soon as it is liberated from the radium. Now in actual practice no such quantity as a gramme of radium has ever been used in these experiments; the usual amounts have varied between a fiftieth and a twentieth of a gramme. Thus the largest volume of niton which was available measured somewhere in the neighbourhood of 0·005 cubic millimetre (0·00000032 cubic inch). It goes without saying that it would be impossible to handle such a minute quantity of gas by any ordinary method; to attempt to transfer it direct from one vessel to another would be a hopeless task. There is a way round this difficulty, however, for if niton be mixed with a large volume of air or any other neutral gas, the air can be passed from one containing vessel to another quite easily, and the niton is carried over along with the air. In this way very trifling loss, if any, occurs during the manipulation.

Like many other gases, niton can be liquefied; but the task is one of the greatest difficulty, owing to the small quantity of material at our disposal. The liquefaction has been accomplished by Ramsay and Gray, who obtained a clear liquid, colourless and transparent when viewed by transmitted light. In the dark, the liquid niton is phosphorescent and shines with a colour which varies with the nature of the glass of the tube enclosing it; the usual tints are green and lilac. When highly compressed, the body exhibits a tint approximating to bluish pink. Ramsay and Gray were even able to prepare solid niton, which melts at about seventy degrees below zero on the Centigrade scale. The solid form of the substance is phosphorescent like the liquid, but the emitted light is much more brilliant. Also, the tint of the light varies

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with the temperature of the body. Thus, when niton is cooled just below its melting-point, it glows with a steel-blue tinge; further cooling deepens the colour to yellow, and a still lower temperature changes the tint to orange-red. On allowing the tube to become warm again, these colours reappear in the reverse order.

We have already mentioned on a previous page that niton belongs to the inert elements. Let us see what evidence this conclusion was based upon. Rutherford and Soddy showed that when niton was passed over red-hot platinum black, or magnesium powder, or zinc dust, no combination took place between the radioactive gas and the metal. Very powerful chemical reagents were also found to be without effect. Ramsay and Soddy tried passing electric sparks through niton and oxygen—a method which, as we have seen, brings even nitrogen into combination—yet no effect was observed. In another experiment they passed niton for three hours across a mixture of lime and magnesium which was heated to bright redness, but the niton remained unaffected by even this treatment. From such results, it is clear that the chemical inertness of niton finds its only parallel in the case of the elements of the argon-helium group, so that it is reasonable to class it along with these bodies.

Before its place in the Periodic Table can be definitely settled, however, we must know the atomic weight of niton: or, in other words, we must find out the relation between the weight of an atom of niton and one of oxygen. Ramsay and Gray attacked this problem; and their apparatus is recognized to be the high-water mark of refinement even among the finest productions of modern mechanism. Some idea of the delicacy of the balance constructed by them will be obtained if we mention that it is capable of displacement from the central position when the minute load of a hundred-thousandth of a milligramme

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is placed on it. This weight in our English measures is only 0·00000004 of an ounce. By means of this extraordinarily delicate machine, Ramsay and Gray were able to deal with a very tiny bubble of niton which weighed only about a fourteen-hundredth of a milligramme (about two-millionths of an ounce), and their result showed the atomic weight of niton to be about 222, so that its place among the elements is that given in the Table at the end this volume.

The spectrum of niton was examined by Ramsay and Collie, who found that in general characteristics it closely resembles those of the other inert gases, such as argon. One bright line in the spectrum appears to be identical with a similar line in the spectrum of lightning which had hitherto been unknown in ordinary spectra.

So far, we have been dealing chiefly with the physical and chemical properties of niton; let us turn to its radioactive character and see how it resembles the other radio-elements. In the first place, niton, like radium, is continuously evolving heat. Both radium and niton throw out rays, but while radium sends out all three types of Becquerel radiations, niton emits only the X-rays. It will be recalled that when dealing with radium we mentioned that its radioactive properties did not appear to be affected by heat; and the same appears to be true for niton. Nor, in the case of niton, does pressure seem to have any influence upon the degree of radioactivity.

We have seen that, chemically speaking, niton is an inert body. It forms no compounds with other elements. But in another way it is a powerful agent in chemical processes. If niton be dissolved in water, the latter compound becomes broken up into oxygen and hydrogen, while if niton be mixed with oxygen and hydrogen it combines those two elements to form water. Thus niton appears to be capable of stimulating chemical action in other bodies while remaining unaffected itself.

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We now come to another property of niton, which has proved to be one of the strangest possessed by it, and we may take up the two lines of evidence which bear upon this point. It was found by Rutherford, M. and Mme. Curie and Debierne, that radium, thorium, and actinium all possess the power of inducing what is called "excited activity" in other bodies. Thus if some radium salt be brought into the neighbourhood of a neutral substance for a short time, and then taken away again, it is found that the neutral body now begins to show signs of radioactivity. This activity, however, is not permanent, but decays regularly until it dies away altogether. Further investigation proved that the amount of activity excited was proportional to the quantity of radium emanation present in the neighbourhood of the neutral body.

To what process can this "excited activity" be ascribed? The answer to this question was furnished by some experiments of Rutherford. He showed that when a platinum wire is exposed to the emanation from thorium, it becomes endowed with excited activity. On immersing the wire in water, no great change in the activity is observed after the wire has been dried again; but if the wire be dipped in concentrated hydrochloric acid, the activity is lost by the wire and transferred to the acid; while if the acid be evaporated to dryness in a platinum dish, it is observed that the dish in turn acquires radioactive properties, and some radioactive matter can be removed from the inside of the platinum vessel by simply scraping it.

All this goes to prove that we are dealing here with some solid, active body which is insoluble in water, but soluble in acid. It is first deposited on the platinum wire, from which water will not remove it. It dissolves in the acid, and is thrown down when the acid is evaporated, forming a film on the platinum vessel. Finally, it can be scraped off the dish mechanically.

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The second line of evidence bearing on this point is to be found in a research of Ramsay and Soddy. They measured the volume of a bubble of niton enclosed in a tube, and found that as time went on this bubble diminished in volume until in about four weeks it had sunk to about four per cent. of its original bulk. Now, this phenomenon can be explained if we assume that the gaseous niton is being changed into some solid substance, which, of course, will occupy much less space than the original gas.

Thus by these two independent methods we are able to deduce that radioactive emanations are gradually depositing a solid body, and further work showed that this active substance was not a permanent material, but changed its nature in time. By a measurement of the rate of decay of the product, it was proved that we were really dealing with several successive phases of decomposition, and in this way the existence of the radium derivatives A, B, C, D, E, and F was established. Their properties have already been summarized in the table given on p. 185.

These results, however, depended to some extent for their value upon the acceptance of certain theoretical assumptions, and for some time the actual rigid proof of such a degradation of niton remained rather in the domain of theory. The quantities of substances produced during the changes were extremely small, and, consequently, it was almost impossible to apply chemical or spectroscopic tests to the materials. The work of Ramsay and Soddy placed the matter beyond doubt.

We have already seen that there is weighty evidence in favour of the idea that niton belongs to the inert gaseous elements, and when it is pointed out that helium generally makes its appearance in conjunction with minerals containing thorium or radium, we begin to trace a connection between these various phenomena. The question

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suggests itself: Is this simultaneous occurrence of helium and the radioactive minerals a mere coincidence, or are the two materials closely interrelated?

Ramsay and Soddy clearly established the connection. They dissolved some radium bromide in water, and collected the gas which was evolved. This proved to be, for the most part, oxygen and hydrogen. These and other common elements were removed by chemical processes, and the final residue was examined spectroscopically. It proved to be a tiny bubble of helium.

In this way it was proved that niton does give rise to helium, and by means of the spectroscope it was actually possible to observe the increase in the amount of helium with the passage of time, as more and more niton became converted into the former gas. In one experiment it was found that, on 17th July, the tiny vacuum tube containing the emanation showed an unknown spectrum, probably that of niton. After standing until the 21st July, the tube gave out the spectrum of helium, which had not been visible in the spectra observed four days earlier.

Thus the degradation of niton into an inactive known body was conclusively established, and we are now justified in saying that radium produces niton, which, in turn, gives rise to helium.

These experiments threw light upon another matter of interest. It has been found that helium exists in several minerals, and also in the waters of mineral wells, such as those at Bath, and previous to the work of Ramsay and Soddy the presence of this gas in these places had been inexplicable. In the light of the Ramsay-Soddy investigation, it seemed of interest to test the waters for radioactive properties, and it was found that both minerals and natural springs of this type were actually radioactive. They probably contain a slight trace of radioactive emanation, which, in its turn, breaks up into helium.

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In the two foregoing chapters we have seen how the science of radioactivity has developed, until it has forced itself into the forefront of chemical and physical speculations. The discovery of radium, with its peculiar and, at first, incomprehensible properties; the proof that it was capable of atomic decomposition, with the liberation of another new element, niton; the further results which showed that niton, in its turn, could yield helium by a process of elemental degradation—all these new facts could not leave either chemistry or physics unmarked by changes in their fundamental conceptions. With so many revolutionary phenomena before the eyes of chemists, even the wildest dreams of the alchemists began to appear commonplace; and it was not long before the possibility of transmuting one element into another began to be discussed as a serious possibility, after centuries of unbelief. The changes of radium to niton, and niton to helium, had shown that such a transmutation was possible; but in these cases the degradation of one element into another was spontaneous, and could neither be arrested nor accelerated by any human agency. It remained still a problem whether or not the chemist could, at will, alter any given element into another. We shall see the fate of the recent attempts in this field in the next chapter.

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CHAPTER XI

TRANSMUTATION

IN the age of alchemy, before the birth of what may properly be termed chemistry, three great problems loomed large in the imagination of students: The Elixir of Life, by whose aid men were to prolong their days and gain continually in knowledge and experience; the Alkahest, the solvent which was to dissolve every material; the Philosopher's Stone, which could change the baser metals into gold, bringing wealth and power to the hand of its discoverer. These three objectives stirred the ambition of many, and though the search was vain, yet the facts acquired in the pursuit served as the foundation upon which our modern chemistry has been laboriously upraised.

At the present day, the search for the Fountain of Youth is not pursued with any great enthusiasm. On the one hand, the higher pressure of modern life tends to make the world's population less keen to extend their days than was the case in the times of yore; while on the other, our knowledge has increased to the stage which makes it clear that we are only in the beginnings of our study of living tissue, and it has consequently become evident that much more data will be required before any attempt is made with any hope of even partial success to attack the problem of the prolongation of life by chemical means.

The Alkahest has also passed out of the world of chemistry. If such a substance were offered to the chemist of to-day, he would probably look on it with mild interest, wondering what sort of vessel it could possibly be

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preserved in, since it dissolved all materials. At the present time we have no lack of solvents, but their chief use lies in the fact that since some bodies are soluble in a certain solvent while others are insoluble, it is possible by using the solvent to separate one class of body from the other. A universal solvent would lack even this advantage.

The problem of the Philosopher's Stone, however, lies in a different category. We are, at the present day, keenly interested in the nature of the elements. We seek for any information that will throw light upon these bodies which serve as the bricks from which our universe is built up; and in consequence the question of the transmutation of one element into another is one of perennial interest.

The problem may be resolved into two lines of inquiry. In the first place, we must endeavour to discover if there is any ground for supposing that such a change of one element into another is possible; and secondly, assuming this to be the case, we must see whether an actual alteration in elemental character can be produced in our laboratories. Let us take up these two sections in turn.

A little thought will show that the spectroscope has enlarged our field of chemical investigation to an almost boundless extent; we are no longer tied down to an examination of the materials of the earth's surface; but we can with almost equal ease make investigations of the compositions of the various bodies which compose our stellar universe. Let us see what results are deducible from such observations.

It is hardly necessary to point out that the generally accepted theory of cosmic evolution postulates a cyclic process in the universe. If we imagine two dark stars, cold like the earth, colliding in space, the result of their impact would be the evolution of a vast amount of heat, for the kinetic energy resulting from their motion would suddenly be transformed into thermal energy, just as is

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the case when a bullet strikes a target and becomes heated. The stellar collision would probably liberate so great a store of heat that the materials of the two stars would melt and volatilize completely; so that instead of the solid globes we should find a gigantic cloud of gas surrounding the spot at which they impinged upon each other. As time went on, the nebula would lose heat to outer space; cooling would set in, with a resulting shrinkage in the volume of the gas. Attraction of particle for particle, now no longer restrained by the dissociating action of heat, would lead to a closer approach of the fragments of matter. A further stage would end in the formation of a new great globe which would grow cooler and cooler with time until at last it became a dark star similar to its two parents. This dark star would then proceed through space and, in time, it might collide with another mass of matter, whereupon the evolution of heat and subsequent processes would run their circle.

Now let us assume that there is no possibility of the transmutation of one element into another, and it will become self-evident that the chemical constitutions of a nebula and its parent star should be the same as the constitution of the new star which results from the cooling of the nebula. We start, say with a pair of stars composed of iron and carbon; the nebula resulting from their collision would also contain iron and carbon, and the star evolved by the cooling of the nebula would in turn be an iron-carbon star.

Thus, on this hypothesis, an examination of the stellar universe by the aid of the spectroscope should reveal to us various classes of stars and nebulæ, each of which classes should contain the same series of elements.

The actual results of our examination, however, would be found to be quite different. The nebulæ and the stars differ entirely in their spectroscopic character; and, further,

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the stars themselves differ from each other. Let us take the main results and see what is deducible from them.

The relative temperatures of glowing bodies may be determined spectroscopically; for it is found that the hotter a substance is, the further towards the ultra-violet does its continuous spectrum extend. Thus iron at a red-heat emits red rays; heated to a higher temperature it emits shorter wave-lengths in addition to the red waves, so that it appears "white hot."

An examination of nebulæ shows that bodies of this class which are in their earlier stages of existence (those where the gaseous mass has not begun to condense), have a spectrum consisting of three lines, one of which is characteristic of hydrogen, while the other two belong to elements which do not exist on the earth. The older nebulæ, in which the gas has begun to contract into a more compact mass, exhibit the spectrum of helium in addition to that of the other three elements known in the earlier nebulæ. Coming now to the stars, we find that they may be grouped into five main divisions. First come what are termed the helium stars, intensely luminous, and containing hydrogen, helium, oxygen, nitrogen, and magnesium, along with traces of other elements. The next series, the Sirian stars, shine with a brilliant white light, and their spectra show the presence of iron. The sun is the prototype of the third class; it contains a very large number of metallic elements. The fourth type of star is termed the Antarian, and in it the elements present in the sun are also found, but titanium appears to exist in the Antarian star to a much greater extent than in the sun. Finally, we come to the carbon stars, which are red, and rather faint in their appearance. In this series carbon makes its appearance accompanied by calcium and other metals.

Now, if we take these data at their face value, it would appear as if there was no relationship between the

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composition of the nebulæ and the composition of the stars; for the nebulæ do not contain many elements which are present in the stars. On the other hand, if we follow the nebular hypothesis, we are driven to conclude that the materials of the nebulæ are the same as the materials of the stars, with this difference, that some of the nebulæ in the temperatures are probably much higher than in the more solid star masses.

Instances are actually known, and have been studied spectroscopically within recent years, in which two invisible dark stars have collided and flashed into vapour on their impact. Such phenomena put our first assumption out of court; we cannot assume that there is a difference in composition between stars and the nebulæ arising from them. What, then, can account for the difference in composition of the stars and the nebulæ? Clearly we must conclude that when the elements of the colder stars are raised to the extremes of temperature which exist in the nebula, the complex elements, such as iron and carbon, disappear as such, and become disintegrated into the simpler substances helium, hydrogen, and other light elements.

Taking this as a working hypothesis, we shall find that there is a steady increase in the complexity of stellar composition as the stars cool down. Beginning with the early nebulæ, we find present only hydrogen and an unknown gas; with the cooling of the nebular mass, as condensation of the gas takes place, helium is formed from the simpler elements; by the time the gaseous material has become consolidated into the form of a hot star, oxygen and nitrogen have come into existence in large quantities; and we can trace the further progress of this synthesis of the elements until at the cooler end of the scale we come to the production of large quantities of carbon in the oldest stars. This hypothesis of stellar evolution is largely due to Sir Norman Lockyer, and it will be seen that it pictures the chemical

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atoms growing during the cooling of the primal ultra-atomic vapours, just as a visible drop of rain grows by the condensation of invisible particles of water-vapour.

The evidence from astrophysics, then, tends to make it appear probable that some elements can actually be transmuted into others. Let us next come to the Earth's surface and see if we can find any analogous process at work there.

We have seen in the foregoing chapters that radium is apparently an element in the chemical sense of the word; it has individual properties not shared by any other element, but it is chemically allied to the metals of the barium group. On the other hand, as we have also seen, it decomposes spontaneously and liberates niton. Thus we have a body which behaves as a chemical individual, but which nevertheless seems to be capable of splitting up into simpler materials. Niton in turn shows a similar series of properties. On the chemical side it resembles the inert gases so closely as to leave no doubt that it is allied to that series of elements, while it readily breaks down with the evolution of helium. There appears to be no doubt that the radioactive elements at least are capable of manifesting the phenomena of transmutation.

Taking these two lines of evidence together, it is impossible to avoid the conclusion that chemical elements are not everlasting; they are capable of modification, and some of them are even so unstable that they become transmuted into others spontaneously. This, however, is only one-half of our inquiry; we have still to discover whether these processes can be set in motion voluntarily by the means which are at our disposal in our laboratories.

It will readily be granted that in order to alter the equilibrium of a stable body, force must be used; and the more stable the equilibrium in which the body lies, the greater will be the force required to shift it out of its position. Now, the majority of the chemical elements are

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evidently extremely stable, since they resist our usual chemical reagents and refuse to split up into simpler compounds. Evidently, then, if the intra-atomic equilibrium of a stable element is to be upset, the amount of energy required will be very great.

What sources of energy have we at our disposal which will enable us to attack this problem? The most important ones which occur to us are the application of heat, the utilization of the forces of radioactive decomposition, and the employment of electricity.

With regard to the application of heat to the problem, the prospects do not appear to be promising. The temperature of the hotter stars, in which the spectroscope leads us to assume the process of atomic evolution, has been estimated as being in the neighbourhood of 30,000° C. Compare with this the temperature of our electric furnaces, which only rise as high as 3000° C. or 4000° C., and we see the gap which lies between our present attainments and the possibilities of transmuting the elements by the aid of heat alone. A temperature ten times higher than we have attained would be essential; and the experimental difficulties in the way of providing apparatus which would stand that degree of heat appear to be insuperable even if the temperature itself could be reached.

The next store of energy upon which we might draw is liberated from the atoms of elements in the state of radioactive decay. This source has been utilized by Ramsay and his collaborators Cameron and Usher. The results are of such interest that it may not be amiss to give a full account of the methods employed.

It will be recalled that when niton is allowed to decompose spontaneously, either alone or when mixed with oxygen and hydrogen, it yields helium. Now, Ramsay observed that when the decomposition took place in presence of water, neon made its appearance, and when the water

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used in the experiment was replaced by a solution of copper sulphate, no helium was detected, but the presence of some argon was observed. Further, traces of lithium were found in the solution of copper after the reaction was over.

A glance at the Periodic Table at the end of the volume will serve to recall the fact that copper and lithium belong to the same family of elements, and that they have been placed together in the same column—Group I.—of the table. It occurred to Ramsay that his results might possibly be accounted for if the heavier element copper had been degraded into the lighter element lithium by the application of the energy liberated during the decay of the niton into helium. A much more rigid series of test was therefore planned.

In order to secure the purity of the materials, the following method was employed. The purest copper sulphate was dissolved in carefully distilled water and the copper was electrolyzed out of the solution and deposited upon a rotating cathode of smooth platinum. Platinum was chosen as it is very resistent to acids. The copper film was washed with pure water and about half a gramme of it was dissolved in carefully purified nitric acid; the solution was evaporated to dryness and the excess of nitric acid was driven out of the crystals by heat. The resulting copper nitrate was then dissolved in distilled water and the solution was divided into two parts. Two bulbs were then blown from the same piece of tubing, in order to make sure that there was no difference in material between them, and the nitrate of copper solution was divided between the two bulbs. One bulb was then charged with niton and had its neck sealed off; the other bulb's neck was also sealed to make all conditions identical, except for the presence of the niton in one bulb and not in the other. The uncharged bulb represents what the chemist calls a "blank experiment."

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After the bulbs had been allowed to stand until the niton in the one had decayed, the copper salt solution from each bulb was transferred to a separate platinum dish and saturated with sulphuretted hydrogen, which has the property of throwing copper out of solution in the form of insoluble copper sulphide. The precipitated sulphide was separated by centrifuging it in a silica tube, silica being used on account of its not being easily attacked by the chemicals employed in the experiment. The solution from which the copper had been removed was then evaporated to dryness and after one or two further operations a spectroscopic examination of the product was made. In the case of the bulb which had not contained niton, nothing was detected; but the residue from the niton-charged bulb showed the presence of lithium.

How is it possible to account for the presence of this lithium? Four different hypotheses suggest themselves. In the first place, it might be due to an impurity in the copper salts employed; secondly, it might have been ascribed to the action of the water or the niton upon the glass of the bulb; thirdly, it might have been introduced accidentally during the course of the analysis after the bulb was opened; or, finally, it might have originated by a transmutation of one of the elements present, just as niton itself arises from radium.

Let us try to test these various suggestions. In the first place, if the lithium had been brought in as an impurity of the copper salt employed, it would clearly have occurred in both bulbs and not in the one only, since the same solution of copper salt was used for both bulbs. Secondly, the idea that it might have been dissolved out of the glass of the bulb, or have been brought out by the action of the niton upon the glass, appears to be negatived by the fact that exactly the same results were found later when bulbs of silica were substituted for those of glass, and also by

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the fact that when niton alone was sealed up in the same glass bulb in another experiment in which no copper sulphate was present, no lithium was detected. The third possibility, viz. the introduction of the lithium by accidental contamination during the process of analysis, seems rather far-fetched when it is recalled that several series of experiments were carried out and yet the bulbs of the blank experiments never showed traces of lithium in them. Had the lithium found its way into the vessels during the latter stages of the processes, it is too much to suppose that it took the trouble always to fall into the solutions which had been in contact with niton, while rigorously avoiding the solutions used in the blank experiments.

This evidence, then, appears to show that the copper salt actually did give rise to traces of lithium when submitted to the action of decomposing niton; and thus the problem of transmutation seems to have been first solved.

Ramsay and Cameron's experiments were repeated by Mme. Curie and Mlle. Gleditsch, who used platinum vessels instead of glass and silica ones; but these investigators found no lithium produced, under these conditions.

The negative evidence in such a case can hardly outweigh the positive. It is very hard to get over the fact that when the same solution of copper salt was divided into two portions, one portion which had been treated with niton gave lithium, while the other portion, untreated, showed no trace of the presence of this element, the more so since, as has been mentioned, the glass of the bulbs does not give any trace of lithium when subjected to the action of niton under the same conditions as before.

Ramsay and Usher, in a further series of experiments, tried the effect of niton upon thorium nitrate solution. One of the products observed by them was carbon dioxide, for whose presence no reason could be found. The action of niton upon compounds of zirconium, silicon, titanium,

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and lead was then tested, and in each case a considerable quantity of carbon dioxide was evolved.

Examination of the Periodic Table will show that these elements all belong to Group IV. of the Periodic System, and that carbon is the lightest element of this family, just as lithium stands at the head of its own class. Thus the degradation of copper into lithium finds its parallels in the break-down of thorium, lead, zirconium, and titanium into carbon.

We have now seen the results of the application of the forces of radioactive decay to the problem of transmutation; let us turn to the final source of concentrated energy which we possess—electricity.

A good many years ago, Professor Collie had observed that, when bombarded with cathode rays in a vacuum tube, several minerals change colour, while at the same time gas is given off. His investigations continued intermittently, and various bodies were examined as to their behaviour under the discharge. The results have proved of the highest importance.

When calcium fluoride was bombarded by the cathode discharge in a highly-evacuated tube, it was found that a certain amount of gas was evolved, and an examination of this gas by means of the spectroscope proves that it contained, among other components, a quantity of neon. Again, it was observed that when glass wool which has been most carefully purified, is subjected to the cathode discharge under very rigorous conditions which preclude the presence of neon in the ordinary way, neon actually makes its appearance in the tube after a certain period. Let us examine the question in more detail, and see if it is possible to trace the source of this neon.

A glass tube furnished with aluminium electrodes was employed as the field of the cathode discharge. It was most carefully cleaned chemically and then evacuated. A

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little hydrogen was next admitted into it, in order to raise the pressure, and allow the discharge to pass more freely between the electrodes. The current was then allowed to pass, and the tube was heated externally. After the experiment was over, some oxygen was admitted into the tube, and a spark passed between the electrodes exploded the mixture of oxygen and hydrogen, forming water. All the hydrogen was thus removed, and the excess of oxygen was got rid of by means of charcoal cooled in liquid air, which has the property of absorbing oxygen. Neon was found remaining in the tube, as it is not absorbed by charcoal under these conditions.

Various sources for the neon might be suggested. In the first place, it might have leaked in from the air outside owing to the tube not being air-tight. This is disproved by the fact that had air leaked in, some nitrogen must have come in along with the neon, but no spectrum of nitrogen could be detected. Since the nitrogen spectrum is easily recognizable, and since the amount of nitrogen in a sample of air is about eighty thousand times greater than the corresponding quantity of neon, the idea of a leak of air into the apparatus may be disregarded.

Secondly, it might be supposed that a trace of neon had become admixed with the oxygen and hydrogen gases used in the experiments. A series of blank experiments, in which much greater quantities of oxygen and hydrogen were used, but in which no cathode discharge was passed, showed that no trace of neon could be detected under these conditions. The neon was observed only when the cathode discharge was used.

Thirdly, the neon might have been assumed to pass through the glass of the tube or to be retained in pores of the glass until driven out by heat. Another blank experiment in which a glass balloon was evacuated and heated to near its fusing point showed that this idea also

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must be abandoned. No neon appeared within the evacuated vessel when the cathode discharge was not used. Further, when the outer tube was surrounded with a jacket containing pure neon at a pressure of half an atmosphere, the quantity of the neon found in the inner tube after the cathode discharge was the same as usual. Since the pressure of neon in the outer jacket under these conditions was fifty thousand times as great as in normal air, one would expect to have found much more neon in the inner tube if it had originated in a leakage from the outside of the tube.

Finally, we might imagine that the neon might be occluded in the aluminium of the electrodes, being torn out by the violence of the cathode discharge. This is proved to be an erroneous hypothesis by the fact that the same results can be obtained when no electrodes are used, the electric forces being concentrated in a spiral of wire wound round the outside of the evacuated vessel.

These results were obtained at University College, London; but while they were in progress a striking independent confirmation of them was found. H. S. Patterson, working in Leeds University, had been carrying out a series of similar experiments; and it was only accidentally that the two investigators discovered that they were both working on the same line. Patterson's results, though his original line of inquiry differed from that of Collie, agreed with those which we have already described. After the publication of Collie and Patterson's results, Masson repeated the experiments in accordance with their directions; and his results completely confirm the work of the two discoverers.

It may be well to summarize the state of affairs at this point. It was found that when a cathode discharge was passed through hydrogen in a highly evacuated tube, neon made its appearance in the vessel. This neon cannot be obtained from the electrodes or from the glass by heating

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alone; and it can be traced even when no electrodes are used. It cannot have leaked in from the outer air, nor from any connection with the pump. It cannot have transfused through the glass of the tube, as it has been shown that glass, either under the action of cathode rays or when heated even to near its melting-point, forms an impassable obstacle to neon. It cannot have occurred as an impurity in the oxygen and hydrogen used in the process. The only conclusion left to us is to assume that the neon has in some way been produced within the tube.

These results in themselves are striking in the extreme; but further discoveries followed which made the matter even more complex.

An apparatus was constructed so that the vacuum tube for the cathode discharge was surrounded entirely by another tube, and the space between the two tubes was highly evacuated. In this way a vacuum jacket was interposed between the cathode tube and the atmosphere, thus making a leak of neon from the air into the inner tube a practical impossibility. After passing the discharge, the usual quantity of neon was detected in the inner vessel. Merely out of curiosity, Collie admitted some oxygen into the outer jacket (which had been evacuated during the experiment). This oxygen was pumped off again and passed into an explosion apparatus. On passing a spark through it, a faint explosion occurred, showing that some hydrogen was present. This hydrogen had evidently been driven through the walls of the inner tube by the tremendous energy of the cathode discharge. The residual oxygen was then cooled with liquid air and absorbed in charcoal in the usual manner. Some gas remained unabsorbed; and an examination of its spectrum showed that it was *mostly helium, with enough neon present to give the neon spectrum.*

The phenomena in the jacketed tube may be summarized

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thus. In the inner vessel, neon makes its appearance in small quantities; the space between the inner vessel and its surrounding jacket, which space was originally evacuated, is found at the end of the experiment to contain quantities of hydrogen, helium and neon.

The question arises, Have these gases been actually produced within the cathodic vessel, and been driven through the glass into the outer chamber? Confirmatory evidence on the former point has been furnished by the work of Ramsay, who has observed that old X-ray tubes, in which the cathode discharge has been frequently passed, contain both helium and neon.

It seems very difficult to avoid the conclusion that the hydrogen gas used in the experiments has actually been modified in some way by the cathode discharge, and that out of its remoulding have arisen the two inert elements helium and neon. On the evidence before us, it appears impossible to find any other solution of the problem. Two absolutely independent investigators, working without knowledge of each other's researches, have arrived at identical results. A third person has repeated the experiments and has confirmed the data fully. Evidently there can be no question as to the facts. Time will show whether any source of the neon has been overlooked; but at the present time it seems difficult to refuse to believe that either the hydrogen may have been transmuted into the elements of the inert gas series, or that the cathode discharge, acting on the materials of the glass vessels used, may have transmuted them instead of the hydrogen.

These facts summarize our knowledge of the transmutation problem at the present day. It may be not without interest to look at them from a broader standpoint.

In the series of experiments by Ramsay and his collaborators, we saw that, under the action of the forces liberated by the degradation of niton, heavy elementary atoms

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of copper and thorium were broken down into lighter atoms of lithium and carbon. Here, evidently, we have a process of atomic analysis. The experiments of Collie and Patterson show the reverse of this process: hydrogen, our lightest element, is being built up into the heavier atoms of helium and neon, so that a process of atomic synthesis is going on under the cathode discharge. Thus the two series of experiments, taken together, give us a process whereby atoms may be built up or broken down according to the method which we employ.

How do these results agree with what we have already seen in the case of the stellar evolution and degradation of elements? It will be recalled that the nebulæ are vast masses of flaming gas; and it seems reasonable to suppose that through these great fire-mists electric storms play on a scale of which we can hardly form even a very incomplete conception. Under the stress of such gigantic forces, the evolution of the elements from hydrogen or simpler bodies might proceed on a vast scale in precisely the same manner as we find on a microscopic scale in the experiments of Collie and Patterson, where the cathode ray disintegrates hydrogen, and reconstitutes the shattered atoms as helium and neon. On the other hand, the clash of two extinct stars, with the consequent evolution of heat as the kinetic energy appears in thermal form, must at once remind us of the liberation of heat, light, and energy during the degradation of the radium atom. Just as a heavy element, caught in the sudden dissolution of a star into a mist of vapours, loses its stability, and swings headlong into a simpler form, so it may be with the stabler atoms when involved in the conflict of atomic disintegration. The stupendous phenomena of stellar evolution are at last within touch of our laboratories, and in a few years we may hold the key to the greatest inorganic chemical problem in the history of the world.

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CHAPTER XII

THE NATURE OF THE ELEMENTS

CONTEMPORARY history is one of the most difficult subjects with which one can deal. After a generation has elapsed, it is possible to detect the really important events, and to separate them from the great mass of facts which are of merely current interest; but when a writer deals with the circumstances of his own time, he is debarred from the aids to a true perspective, and his work becomes of a more speculative nature than is the case when he treats of a period which later history has brought into its real proportions.

This is especially true of the history of science; for all scientific theories are avowedly only a series of approximations to the truth, and the progress towards the goal may take the form of a zigzag rather than a direct line. The theory which to-day appears to fit the facts of experience may prove to-morrow to be ill-founded, and incapable of being brought into agreement with further discoveries.

Nevertheless, at the present day, we can foresee to some extent the view which the historian of the next generation will take of the state of chemistry at the beginning of the twentieth century. He will look back upon this time as one in which the old and apparently well-founded ideas of the nineteenth century began to lose their definiteness, in which the newer discoveries at one sweep overwhelmed the very foundations of the science, and in

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which the newer views were so strange that it became a matter of the greatest difficulty even to find definitions for the conceptions which had taken the place of the older beliefs. The discovery of radioactive phenomena has undermined the whole structure of the physics and chemistry of yesterday, and at the present time we are hurriedly endeavouring to dismantle the older edifice while utilizing as much of its material as is suitable in the construction of a more modern erection. Our ideas of the fundamental structure of matter itself have undergone a complete alteration, and even the conception of the chemical elements has suffered a kind of disaster in the course of the last few years. It seems not without interest to follow the general trend of this line of thought, and to try to gain some idea of the position in which we now stand.

Let us begin with a very simple example. On mixing oxygen and hydrogen together in certain proportions, and then passing an electric spark through the mixture, the gases can be made to unite, yielding water as a result. From this we deduce that water contains both oxygen and hydrogen. It is therefore not a simple body, but a compound of two different substances. By placing the poles of an electric battery in the water, we are able to decompose the liquid into the two gases oxygen and hydrogen. Thus what we call a compound is a substance which can be built up from simpler bodies, and which, in turn, can be decomposed again into its constituents. Now apply the same idea to oxygen. The chemist tells us that up to the present he has discovered no method whereby he can produce oxygen from simpler bodies, or decompose it into other substances. Oxygen, then, is not a compound as far as we know, and we give the name "elements" to the class of substances to which it belongs.

At the present date chemists are acquainted with over eighty of these elementary bodies, each of which differs

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from the others in its physical and chemical properties, and none of which can be decomposed into simpler substances by any ordinary chemical process. Evidently these eighty odd elements form the groundwork of matter as we know it; the materials with which we come into contact in our existence are either these elements or are made up of several elements combined together to form a compound. Any knowledge as to the constitution of matter in its ultimate form must therefore be acquired through a study of the elements and their properties.

Chemical investigation tells us that the elements differ from one another to a greater or less extent; some elements are very reactive, like phosphorus, others are inert like argon, and refuse to combine with other elementary bodies. But we cannot assert that this difference corresponds to a difference in their ultimate composition. There are obviously two possibilities before us. On the one hand, the element sulphur may be a peculiar form of matter which in no way resembles the matter which composes the element argon, for instance; or, on the other hand, all the elements may be built up from a common material which is utilized differently in the case of each elementary body. For example, if we take four billiard balls we can arrange them either as a square or as a pyramid; the properties of the square and pyramid differ from each other just as much as the properties of sulphur differ from those of argon, and yet both square and pyramid are built up from the same materials, the four billiard balls.

Which of these two concepts is the more probable? Owing to a peculiarity of the human mind the idea of uniformity is always more satisfactory than one of discontinuity, for by a survey of a process carried out in a uniform fashion we are able to trace connections and draw general conclusions as to the forces in operation, whereas a series of disconnected facts or catastrophic upheavals

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suggests to our mental machinery nothing which we can weave into a co-ordinated whole. We are, therefore, quite unconsciously led to favour a "uniform" idea of most phenomena, and it is not strange that even the early Greeks decided in favour of a primordial material from which the various types of concrete matter were built up. The fact that these philosophers, with nothing but the most rudimentary data at their command, persisted in constructing their theories of the universe merely shows how ingrained in the human mind is this idea of continuity.

Scientific progress, however, appears to be on the verge of establishing the truth of these bold speculations. Within the last decade the advance in our knowledge of the ultimate constitution of matter has attained to such a degree that it is possible at the present time to take the "uniform" view of matter without running the risk of hasty generalization. It may not be without interest, however, to trace the rise of the idea, and show the various stages through which it has passed.

Leaving aside the earlier speculations which had no experimental basis, the first real step was taken by Prout almost exactly a century ago. Basing his conclusions upon the relative weights of atoms as they had been established in his day, he assumed that hydrogen was the primordial form of matter from which all the other elements were built up. Thus an oxygen atom, which is sixteen times heavier than a hydrogen atom, might be supposed to be constructed from some peculiar arrangement of sixteen hydrogen atoms which, in the course of this combination, had lost the properties of hydrogen and acquired other properties characteristic of the whole larger grouping. A sulphur atom, being thirty-two times as heavy as a hydrogen one, would contain thirty-two atoms of hydrogen.

This hypothesis of Prout had much to recommend it upon the score of simplicity, but unfortunately later and

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more accurate investigations of the relative weights of elementary atoms destroyed it entirely. Thus, if we take the atomic weight of hydrogen as unity, we find that the weight of any oxygen atom is 15·88, and not 16, while the atomic weight of sulphur is 31·82, instead of 32. Obviously, then, an oxygen atom cannot be built up from an integral number of hydrogen atoms: and so Prout's hypothesis fell to the ground. Attempts were made to revive it by assuming that the unit of matter was a fraction of a hydrogen atom, but this conception militated against the simplicity of the theory and led to so many assumptions that it failed to recommend itself to the chemical world.

Nevertheless, though the unitary conception of matter had thus received a set-back, it soon revived in a modified form. Instead of the assumption that the various forms of matter, which we know as elements, were built up from other known elements, the new form of the theory took as a basis the existence of a substance "protyle," which may be assumed to exist only in various complex groupings, each of which corresponds to one of our known elements. In this guise, no objection could be taken to the theory, but at the same time nothing much could be hoped from it; for so long as protyle remained undiscovered, we were no further forward for any practical purpose.

About this time, however, researches in spectrum analysis opened up a new line of evidence bearing upon the question. It was found that each element when heated to a sufficiently high temperature was capable of emitting a spectrum of lines which was perfectly characteristic. No two elements gave the same spectrum; and the spectrum emitted by a given element was always the same, just as a tuning fork always gives out the same note. At first sight this evidence does not seem to favour the unitary theory of matter, but rather to contradict it. Further research, however, threw a somewhat different light upon the

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question. Some elements, such as iron, have a very complex grouping of lines in their spectra; and as these lines must be produced by the vibration of particles, it follows that in the electric arc the atoms of iron must be in a state of violent perturbation. A closer study of the spectra shows that in the case of some elements the lines can be grouped into classes which bear a simple relation to one another. The keyboard of a piano furnishes us with a rough simile. If we start at any note we can go up the keyboard and play seven different notes in succession; our next note, however, will be the octave of the first, and the ninth note will be the octave of the second. Thus by the time we have played fourteen notes we have completed two series of seven notes each, and the second seven notes are closely related to the first seven. The same type of phenomenon is traceable in the lines of the spectrum; the spectrum of sodium, for example, contains three distinct series of lines. Now an examination of these series brings out the fact that the periods of vibration of the particles causing them are not the same; in other words, a single atom cannot be the cause of all the lines, but they must originate in the motions of several different particles. But if this be granted, then obviously the atoms of our elements are not single particles, but are built up from still finer bodies.

More recent investigation has confirmed the truth of this idea. Light being an electro-magnetic phenomenon, it seemed possible that peculiar effects might be observed if an examination were made of light which had been produced in a very strong magnetic field. Zeeman undertook this line of investigation, and his work resulted in the discovery of what is now known as the Zeeman effect. An electric arc is placed in a position that permits a strong magnetic field to be concentrated upon it. The spectrum of the arc is examined and certain lines are observed by means of the spectroscope. The magnetic field is now

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brought into play and the spectrum of the arc is again investigated. It is then found that a marked change takes place in the spectrum. In the original spectrum, for example, there may be three lines at particular points; but when the magnetic field is employed it is found that one of these has split up into two new lines, another single line has been replaced by six new lines, while the third original line has remained unaffected. If the three original lines were produced by the vibrations of a single particle it is clear that, when the magnetic field was applied, this particle would take up a new period of vibration, and consequently all three lines in the spectrum would be equally affected, yet, as we have seen, some of them remain unaltered while others split up into new lines. Clearly, the only conclusion to be drawn is that the original three lines were not produced by the vibrations of a single particle, but are to be attributed to the motions of three separate particles which are differently influenced by the magnetic field.

With this evidence before us, we cannot avoid the assumption that atoms are not the ultimate basis of matter. Below and within the atom lie a series of sub-atoms or discrete particles, each of which is capable of vibrating with a fixed period of its own, and until we are able to decide what the nature of these particles is, we shall not be near the solution of the problem of the constitution of the elements.

A further step in advance has been taken in recent years, and since the beginning of the present century our ideas as to the constituent particles of ordinary elements have been considerably clarified. In previous chapters of this volume we have seen the close connection which exists between electricity and radioactive phenomena. We know that particles of electricity—electrons—are shot out from the cathode of a Crookes tube, and we have learned also

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that similar particles are driven off from the atoms of radioactive elements in the process of decomposition. Further, we have seen that these electrons are probably extremely small, much more minute than any particle of what we term matter. The question suggests itself to us: Are these electrons the ultimate constituents of our material universe?

It appears probable that this question may safely be answered in the affirmative in a limited sense. We have seen that electrons are particles of negative electricity; but ordinary matter is not negatively charged. It is evident that matter, in addition to electrons, must have another constituent which serves to neutralize the negative charges of the electrons. In other words, an atom must contain both positive and negative charges; and the usual assumption now made is that put forward by Sir J. J. Thomson, viz. that an atom is built up from negative electrons conjoined with a positive sphere.

This hypothesis serves to explain the most varied phenomena. It accounts for the complexity of many spectra; and by its aid we can comprehend the meaning of the Zeeman effect; while it has also been pressed into service to throw light upon the problem of the absorption of light by elements and compounds. On the physical side it enables us to interpret the phenomena of radioactivity on a simple basis; it throws light upon the relations between electricity and matter in such fields as the ionization of gases. Applied to chemical problems, it has also borne fruit in some lines of inquiry; and further advances may be anticipated in the future. With such a series of successes to its credit in such widely separated fields, it must be admitted that there is a strong probability that the electronic theory of matter, if not the whole truth, is at least a very close approximation.

Let us now look more closely into one or two points

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connected with the theory. Take, first, the question of the lines in the spectra of various elements and the phenomena of the Zeeman effect. It has already been pointed out that in order to produce the numerous lines of the iron spectrum, for example, there must be a number of particles in motion within the iron atom, and we have identified these particles with the electrons which constitute part of the atomic structure. But this conception takes us a step further in our inquiry, for it shows us that we cannot regard the intra-atomic structure as a series of fixed particles; we must picture it rather as a series of minute bodies in very rapid motion. In other words, the electronic structure of an atom must resemble, to some extent, on a very minute scale the state of things which we see in the solar system. Looking at the matter from another point of view, this seems a most natural assumption to make, for it is almost impossible to conceive equilibrium existing among a series of particles unless they are endowed with a kind of planetary motion.

Turning now to the chemical side of the question, we find that this throws a further light upon the problem. We know that two atoms may combine together chemically, and we have learned also that in some cases, as in solution of salts in water, two atoms may separate from one another, and in so separating may acquire opposite charges of electricity. Now a charge of negative electricity on the electronic theory corresponds to the presence of an electron upon the charged body: so that we are driven to conclude that chemical combination in its turn may be due to some alteration in the number of electrons in the atoms which are concerned in a reaction.

Modern chemical theory interprets the matter in the following manner. When two atoms unite together chemically, electrons are supposed to pass from the first atom to the second, and each electron bears with it what

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is termed a Faraday tube, or tube of force. This tube of force then unites the two atoms. The process may be likened to the ordinary method of sending a line to a wreck by means of a rocket: the land represents one atom, the ship plays the part of the second atom, the electron is the rocket, and the tube of force the line carried by the rocket from the shore to the wreck. We know, however, that by means of a second reaction we can split apart the two atoms and unite each of them with fresh atoms; yet even after this drastic treatment the atoms do not change their elemental nature. Thus atoms may lose a certain number of electrons to other atoms and take up fresh electrons from a second set of atoms without losing in any way their original attributes. Clearly, then, there must be two series of electrons within our chemical atom. One of these groups of electrons is a stable arrangement which remains practically unaltered through all chemical reactions; the other collection of electrons comprises those which are capable of wandering from atom to atom and whose movements produce what we term chemical changes in matter.

There is a third standpoint from which we may regard this subject. We have seen in the foregoing chapters that the radioactive elements are in a state of spontaneous and continuous decomposition, accompanied in many cases by the liberation of electrons. Clearly, then, since these elements are all comparatively massive in their atoms, it is possible to deduce that there is an upper limit of atomic stability. When the atom becomes extremely complex in its structure the electrons composing it evidently interfere with each other's vibrations to such an extent as to render the whole structure too unstable to remain permanent. A rearrangement takes place; some electrons are expelled, and the remainder settle down into more stable equilibrium. Thus atomic structure to some extent resembles a house of

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cards. One or two stories may be safely built and the erection will not topple; but as more and more cards are built into the edifice it becomes less and less stable until finally the slightest interference serves to bring it to the ground.

It will now be clear that our ideas of the elements are becoming more complicated with the advance of science. In the earlier days of chemistry, elements were assumed to be particular types of matter; the atom of an element was supposed to be the most minute form of matter which could exist; and the possibility of converting one element into another was supposed to be beyond our reach. Within a very short period, all this has changed. The atom, instead of being the simplest form of matter, is found to be an extremely complex system of infra-atomic particles; matter and electricity have been found to have an exceedingly close relationship with one another, and the conversion of one elementary body into another is no longer looked at askance.

There is a final complication of the problem of the nature of the elements which we have not yet touched upon. Hitherto we have assumed that when we speak of an atom of hydrogen we mean one particular thing, something about which we can be perfectly definite. But are we quite justified in this view? When we speak of an element, we mean something which presents to our physical apparatus and chemical reactions a certain series of properties of the element. But we know that the material which we employ in any of our experiments probably includes millions of atoms, and our apparatus does not permit us to ascertain the properties of one of these individual atoms. All that our observations can give us is the *average* of the properties of these various atoms, and it is this series of *average* properties which we term the properties of the element in question.

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A somewhat absurd analogy will throw this into relief. Suppose that we were shown four men and asked to draw up the description of their average properties. We might say that the average man had two arms, two legs, two hands and two feet, a head with two eyes, as well as a body. It would probably be impossible for us to include the fact that one of the men had a squint while another was so unfortunate as to possess a broken nose. In the same way, our dealings with atoms are carried on upon such a wholesale scale that we have no means of taking into account the peculiarities of individual atoms; we can only note the broad similarities which run through the whole mass of atoms with which we are dealing.

Our chemical reactions are certainly a very refined way of sifting one class of atom from another class; but they can make no pretence to separating one atom from another when the two atoms belong to the same elementary class. No reaction with which we are acquainted will separate one sodium atom from another and enable us to say that one has peculiarities which the other has not. We are rather in the position of a person separating gravel into classes by means of two sieves: when he has riddled the heap through a one-inch sieve and a two-inch sieve he can say that the stones in one heap are less than an inch in diameter, those in a second heap are between one and two inches in diameter, while the third heap contains stones of over two inches in diameter; but in the one to two inch classes there are all sorts and sizes of stones whose dimensions lie anywhere between one and two inches; only a rough separation has been made. So it is with our chemical reactions.

It seems very improbable that we shall ever be able to deal with individual atoms; the experimental difficulties which can be foreseen in connection with apparatus which in itself will be composed of atoms, are too great to be

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overcome by any methods which we can contemplate. But it is quite within the bounds of possibility that as science advances our methods of separating one class of atoms from another may become more delicate and refined; so that what we call elements to-day may be shown to be mixtures of two or more simpler bodies. This has already occurred in the past; and even at the present day in the class of the rare earths it is becoming quite a common occurrence to find that a substance which seemed at first to present all the characteristics of an element is really a mixture of two constituents.

This completes the survey of the constitution of the atoms; and it will be clear that what at first seemed to be a very simple problem, has in recent years developed and become more and more complicated. Evidently we are only part of our way on our journey towards a knowledge of the organization of matter; yet the strides which have signalized the opening years of the present century are sufficient to mark that period as one of the most interesting in the history of chemical science.

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CHAPTER XIII

CHEMICAL PROBLEMS OF THE PRESENT AND FUTURE

LOOKING at the complex fabric of modern civilization, one is often tempted to inquire which parts of it are essential and which factors could disappear without dislocating the whole machine. At first sight it seems an easy task to put one's finger upon certain features and say: These are luxuries, and might be abolished without causing any vital change in essentials. But such a criterion is very hard to apply with any real results. For example, if we suppose that a dinner at the Carlton Restaurant is a luxury, and proceed to abolish that establishment, we shall simply produce a bigger clientèle for the Aerated Bread Company. Or, if we classify a Rolls-Royce car under the head of luxuries, and force its owner to dispense with its use, we shall merely drive him to reach his business in some other way, by means of a cheaper form of transport. It is evident, then, that this method of elimination will not help us much; we must approach the matter from another point of view. Instead of saying that such and such a form of a thing is a luxury, we must ask ourselves what are the essential factors at the back of present-day social machinery?

Speaking from the purely "mechanical" point of view, it is clear that there are two things which must inevitably be present unless our whole contemporary machine is to

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break down. In the first place, we must be prepared to feed our population; and in the second place, we must provide them with sources of power which they can apply to various necessary operations. Given food and power, the mechanical side of our civilization could always exist much as it does at present; but the disappearance of these agents would entail a complete collapse of the whole fabric.

At first sight, neither of these factors appears to have a very close connection with chemistry; yet in actual practice at the present time it is found that chemical methods and research are playing greater and greater parts in the maintenance of these two pillars of society.

Let us take first the question of power. Very little thought will show us that at the present day our main source of power is coal. We submit coal to the chemical process of oxidation—or in plain English, we burn it—and the heat liberated in this reaction forms a supply of energy which can be utilized for the most varied purposes. Coal, then, is a reservoir of what is called potential energy, that is to say, energy which will lie dormant under ordinary conditions, but which can be set free by appropriate methods. Let us see if by going further back we can trace the origin of this energy. Geology tells us that coal was produced from the tropical vegetation which flourished in the area of the coalfields during what is termed the Carboniferous period; so that evidently the energy locked up in coal was also present in the trees and plants of that age. Now, plants in general require the aid of sunlight for their growth; and it is manifest that some of the energy of sunlight is used up in the process of plant development; so in this way it becomes clear that the fire in our ordinary grate owes at least part of its energy to the sunbeams of the Carboniferous age.

This line of argument shows that when in ordinary

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language coal is spoken of as a "source" of power, this really implies that it is the last link in a chain of transformations of energy which have been going on throughout countless centuries in the past history of the earth; and it becomes evident that for practical purposes two factors are essential in the power problem. In the first place, a source of power is needed, and in the second place there must be one or more intermediate links in the chain of energy transformations ending with a final substance which will liberate energy under conditions suited to our needs.

With regard to the ultimate source of power, there seems to be no doubt that, under present conditions, reliance must be placed upon solar heat, or at least on solar energy of one sort or another. A few examples will show how intimately this energy is already associated with our present sources of supply. Leaving the question of coal, there remain only two other sources of energy which are employed to any extent at the present day: wind-power and water-power. Wind-power serves to transport all the sailing-ships that remain; and in addition to this, in some countries like Holland, it is still employed to work windmills. Now, the power of wind is simply due to what are known as convection currents in the atmosphere. If we look at the air above a gas-jet, we see that it is violently perturbed; the flame of a match burning in strong sunlight will cast a shadow in which the hot air ascending from the flame can be clearly seen. This ascending hot air leaves behind it an empty space into which the surrounding air rushes to fill up the partial vacuum, and so on a small scale we get the phenomena of winds in the atmosphere. The solar rays heat up certain portions of the earth's atmosphere, and the warm air, being lighter than the rest, rises upwards, while cold air rushes in to fill its place. Thus when we utilize the energy of the winds, we are merely employing solar energy under another form.

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Turn now to the question of water-power, and we find the same factor again prominent. Water-power is obtained by allowing water to flow from a high level to a lower one and utilizing some of the energy thus liberated. Evidently the real origin of this power is the force which transfers the water from sea-level to the higher ground; and this energy is obviously of solar origin.[1] The sun shines on the sea and its heat causes a certain amount of water to evaporate and form clouds; these clouds then condense as rain or snow, and part of their condensation takes place on high ground; the total result of this process is clearly the transference of water from a low level to a higher one, and the reverse process is going on when a mountain stream falls towards the sea. If we interpose a mill-wheel in the latter section of the circuit, we are able to tap some of the solar energy which originally raised the water to the clouds.

These instances show that our three main sources of power at the present day are merely somewhat haphazard and expensive ways of trapping some of the energy of the sun. Let us now turn to the second factor in the problem—the substance which is to provide us with an easy means of liberating energy as we require it.

At first sight there appear to be almost innumerable ways in which energy can be transferred from one point to another; but on further consideration two methods stand out above the rest. Energy may be carried in the form of an electric current along a wire, or substances which have been heavily charged with potential energy may be transferred to some position where they will liberate that energy in the manner most convenient to our purposes. The electrical method of power transmission may be a purely physical question, into which chemistry does not enter, for it is possible to derive energy from water-power in a mill,

[1] Of course the force of gravitation is the ultimate factor in this question.

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and then, by means of a dynamo, convert that into electrical energy, which can then be distributed along wires to any required spots. But at the present time a more generally accepted method is to employ as an intermediary what is called a secondary battery or accumulator in which to store electrical energy; the accumulator can then be transported to the spot where the energy is required, and thus long lines of insulated wire are dispensed with. At this point, however, chemistry enters the field; for the processes which take place within the accumulator are chemical reactions, so that this method of power distribution is chemical in its essentials.

The second method of power distribution is purely chemical. Substances such as coal, oil, or wood all contain a very large amount of potential energy, and these reservoirs of energy may be transported to the spot where we need the power; then by starting appropriate chemical reactions we are enabled to liberate the potential energy.

Looking at the general trend of things it is clear that, sooner or later, all our present supplies of coal will be used up; and in the meantime it will be necessary to devise some means of obtaining energy which will take the place of this source. Evidently it will be necessary to fall back upon the great natural forces; solar or tidal energy will have to be harnessed to our needs; and without any great stretch of the imagination the time can be foreseen when windmills, in an improved form, will again make their appearance on the countryside, while Central Africa will be furnished with methods of trapping the solar radiation, and countries such as Norway, where water-power is abundant, will be regarded as the great energy-producers of the world. Tidal engines will also be necessary if all available supplies are to be utilized.

But even assuming that we have this power in

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abundance, we have still two further factors to devise before we could utilize it. In the first place, a method of storing the acquired power is needed; and secondly, methods are needed for distributing energy to the various points where it may be utilized.

With regard to storage, the germ of an apparatus already exists in the form of the ordinary electric accumulator; but in its present state this is a most wasteful machine. Many attempts have been made in the past to produce either a primary or secondary battery which will yield up a reasonable quantity of its potential energy; but hitherto the success has not been more than moderate. As these batteries depend to a great extent upon the chemical reactions which go on within their cells, we must look to chemistry to provide us with improvements in the future.

There is another manner of storing energy, however, which must not be overlooked. Substances such as coal contain a large amount of potential energy which can be utilized—though at the present day our methods of employing this stored-up energy are wasteful in the extreme. Now suppose that, by utilizing natural forces, it were possible to produce a kind of artificial coal; then this new substance might be employed as a convenient source of power. A forecast of this method is already in existence, though at present it is not employed in practice, owing to the cheapness of coal.

We have already[1] described how calcium carbide is manufactured on a commercial scale by means of the electric furnace. Acetylene gas, obtained by the action of water upon calcium carbide, is not only a good source of illumination; it is also capable of yielding an explosive mixture with air, as was unfortunately demonstrated many times over when carbide first came into common use. It

[1] See Chapter III.

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is clear that calcium carbide might therefore be regarded as a kind of artificial coal, from which energy could conveniently be derived, and which could be transported from its place of manufacture to any required spot before liberating the energy which it contains. Acetylene gas, employed in a gas-engine, however, has certain drawbacks which render it rather undesirable at the present time; but it will be seen that in calcium carbide we have the germ of future possibilities; and that sooner or later substances of this type may be utilized as a means of storing and distributing energy derived from sources like water-power.

Following this line of thought, we are led to consider other similar methods of storing energy. Of these, the simplest appears to be in the form of safe explosives. It is, of course, out of the question to drive a motor by injecting a few drops of nitroglycerine into the cylinders; but the same end might be attained in another way. Suppose that a charge of explosive be detonated in an extremely strong cylinder. The pressure inside the cylinder will rise enormously, and there seems to be no valid reason why some simple means should not be devised by the aid of which we could utilize the energy represented by this pressure. It would be possible to lower the pressure gradually and utilize the escaping gases to turn machinery. The loss of energy in the form of heat would certainly be difficult to avoid, but there seems to be no doubt that something in this direction might be attempted. As soon as the engineer has devised the machinery, chemistry will have no difficulty in producing the explosive best suited to the case.

In the above paragraphs we have dealt entirely with the sources of energy which are utilized at the present day; but there is a further reservoir of force which we can look forward to in the future, though it is still beyond our

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reach. All the methods of transforming energy which we have described are what we might term molecular or inter-molecular sources. They are brought about by the exchange of certain atoms for certain other atoms in compounds and do not affect the internal economy of the atoms themselves to any extent. Recent work on radioactive elements, however, as we have seen, has brought before us the possibility of actually breaking up atoms themselves; and such an atomic disruption would yield to us an amount of power incomparably greater than anything which we have now at our disposal. Such a break-down is already going on in the radium of our laboratories, and when it is recalled that the energy thus liberated is projecting α-particles from the radium atom with a velocity of 20,000 miles per second, we have some idea of the forces at work. If we had some means of producing and controlling such a decomposition in the atoms of ordinary elements, we should have at our service a power which would enable us to dispense with all other sources of energy. This, however, is a mere dream of the future; for its realization we must look to the efforts of those who, at considerable risk to themselves, are investigating the properties of the radioactive elements which we already know.

Mechanical power, however, is, after all, not an absolute essential to humanity, though the modern man would feel its loss acutely. Isolated upon a barren island, the latest product of civilization would no doubt prefer a supply of sandwiches to all the machines in the world; for food is the real necessity, without which we cannot subsist.

Let us, then, leave the question of power, and turn our attention to the more fundamental problem of the food-supply of the world. The earth's population, as a whole, appears to be on the increase, and it is evident that in the future we shall have to look forward to an increased food

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consumption on the planet. Now, this can be met in either of two ways: we may endeavour to increase our present supplies, or we may seek for new sources of nourishment. Let us see what chemistry is doing for us in these fields at the present day, so that we may make a guess at future developments.

For the sake of simplicity, let us begin with the problem of increasing our present resources in the matter of food-stuffs. It is common knowledge that a human being is unable to derive nourishment direct from the soil in the same way as plants: our vital machinery is not constructed to this end, but is intended to be merely a means of transforming the protoplasm of plants and animals into human tissue; and it follows from this that if we wish to increase our range of food-stuffs, we must keep within the limits of plants and animals. But animals in their turn are dependent upon plants for their existence, so that the final factor in the problem of human nourishment appears to be the plant. Let us examine the question of plant growth rather more closely.

All organized tissue, when it is analyzed, is found to be built up from the elements carbon, hydrogen, oxygen and nitrogen. Where can plants obtain these elements? Carbon and oxygen they might extract from the carbonic acid gas which is present in the atmosphere; hydrogen might be taken from water; and nitrogen might also be supposed to be derivable from the air, of which it forms a preponderant ingredient. Investigation shows, however, that though some aerial nitrogen may be utilized by plants, their major supply of this element is drawn from the soil by means of their roots. Our problem of food-supply is thus seen to be closely connected with this source of nitrogen, and we must look into this point in more detail.

In the first place, let us find out whence the nitrogen in the soil is derived. Some of it, we discover, is obtained

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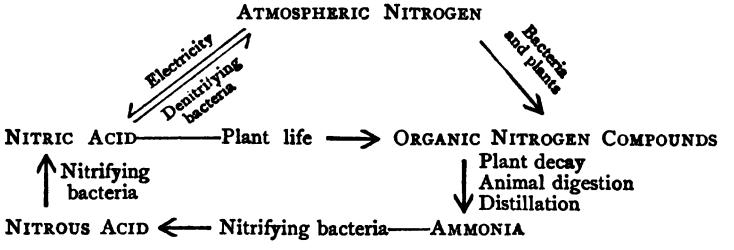
from the air by the activity of what are called "nitrifying bacteria." The greater part, however, is found to originate in the decay of plant tissues in the earth, or to be traceable in one form or other to plant life. A plant may rot as it dies; and in this case its nitrogenous tissue will partly disintegrate into gases, some of which contain nitrogen, while the rest of the same element will remain behind in the soil in the form of various nitrogenous derivatives. Again, a plant may be eaten by an animal. A certain amount of the original plant's nitrogen is then excreted by the animal in the form of urea derivatives and the like. These compounds are broken down in part into ammonia; then certain bacteria come into play whose function it is to oxidize this ammonia to other nitrogen compounds, called nitrous and nitric acids. These acids form salts which, in their turn, may be absorbed by plants, so that we have a cycle of transformations of nitrogen going on in nature. The various steps in this cycle can be seen at a glance from the following scheme:—



An examination of this diagram shows that the main source of nitrogen is the atmosphere. This atmospheric nitrogen is seized upon by bacteria, and by some plants, and through their agency it is transformed into various organic compounds of nitrogen. Then, either by plant decay or animal digestion, or by the action of heat, these organic compounds are decomposed, and the major part of

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their nitrogen content is broken up into ammonia or its salts. At this stage in the cycle, the nitrifying bacteria come into play, and by their oxidizing action they transform the ammonia into nitrous and nitric acids. Part of these substances is utilized by plant life, while the remainder is subjected to the action of a second set of bacteria, whose powers are the reverse of the nitrifying bacteria. These new bacteria have the power of breaking up nitrogen compounds and liberating elemental nitrogen, which finds its way back into the air. The only other factor in the circuit is electricity, and it apparently plays but a small part. During a thunderstorm, for instance, some of the nitrogen of the air may be forced to combine with oxygen to produce nitric or nitrous acid.

Now, it is clear that if a few plants and animals were enclosed in a bell-jar, the nitrogen cycle might run its course for an indefinite time without any appreciable loss; but on the surface of the planet things do not evolve in this ideal manner—waste is sure to take place at some point or other. Let us see where such waste might occur. We are, of course, using the term "waste" in the sense that the nitrogen ceases to be useful to us in the form of food-stuffs.

In the first place, we must not forget that though the vegetable kingdom as a whole is of use to us in so far as it keeps renewing our supply of oxygen, it is only certain groups of plants which are edible. Any nitrogen, therefore, which is absorbed by the non-edible plants is—temporarily, at any rate—lost to us from the point of view of food. Then, again, our sewage is to a very great extent made up of excreted nitrogen compounds, and much of this is run into the sea, where it goes to build up sea-plants, for which we have no present use. Here we have quite a serious loss of nitrogen. Further, the nitrous and nitric acids which are produced by the nitrifying bacteria are

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not totally absorbed by plants; some portion of them is dispersed and lost, as far as practical purposes are concerned. These instances are sufficient to show that we are steadily using up the available nitrogen, and it becomes clear that means must be found to replace this deficit.

Two means suggest themselves from an inspection of the nitrogen cycle. With the first of these—an increase in the nitrifying bacteria—we need not deal, as it lies outside the purely chemical province. The second method is to supply the soil with nitrogen compounds which the plants can assimilate, and this corresponds to the usual practice of manuring. Now, manures may be either natural or artificial. At the present day, the demand for nitrogenous matter far exceeds any possible supply of animal or vegetable manure, so that we are thrown back upon inorganic sources.

It is clear that in view of the enormous quantities of material required, any artificial manure must be comparatively cheap if it is to be of much practical service, and when we bear this in mind we find that there are only two sources from which we can draw. In the process of gas manufacture large quantities of ammonium salts are liberated, and as these are merely by-products, they are comparatively inexpensive. This gas-works ammonia is one of the artificial manures which can be employed. But although the amount of it produced annually is very large, still it would be totally insufficient to supply the world's demands. Fortunately, however, there is a still vaster quantity of nitrogen compounds available to us at the present day. In Chili and Peru there are enormous natural deposits of sodium nitrate, which is termed "Chili saltpetre." This substance is a nitrogen derivative, and it has proved itself to be a most efficient manure. In 1873 these nitre beds extended over areas of thousands of square

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miles, and were estimated to contain some millions of tons of nitre per square mile, so that it might be thought that we had here a practically inexhaustible supply of nitrogen in a form which we could utilize. Some idea of the immense demand for nitre may, however, be deduced from the following figures. In 1896 the entire consumption of Chili saltpetre was 1,060,000 tons, while by 1905 this annual drain had risen to 1,567,000 tons. Some 300,000 tons of this is devoted to the chemical industries, but the remainder, say 1,200,000 tons, is utilized for agricultural purposes. If the figures for the present day were available, it would be found that there has been a great increase on those of 1905, for much more extensive use is now being made of this nitre as a manure. It is evident that with such gigantic annual drafts upon them, the Chilian nitre beds cannot last for any great length of time before they become exhausted, and some modern calculations place the date of their end as early as 1923. What is to take the place of nitre when these deposits are worked out?

Various suggestions might be put forward as solutions of this vital problem. In the first place, since a great portion of the present loss of nitrogen is due to our wasteful method of sewage disposal, it seems clear that sooner or later means will have to be taken to find some less expensive mode of getting rid of these by-products of sanitation. Sewage farms are not pleasant neighbours, but we shall probably be driven to utilizing our sewage on land rather than wasting it in the sea. Few people have any idea of the waste which is at present going on; no less than £16,000,000 worth of fixed nitrogen is annually hurried down our drains and watercourses into the ocean. In the words of Sir William Crookes: "This unspeakable waste continues and no effective and universal method is yet contrived of converting sewage into corn. Of this barbaric waste of manurial constituents, Liebig, nearly

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half a century ago, wrote these prophetic words: 'Nothing will more certainly consummate the ruin of England than the scarcity of fertilizers—it means a scarcity of food. It is impossible that such a sinful violation of the divine laws of nature should for ever remain unpunished; and the time will probably come, for England sooner than for any other country, when, with all her wealth in gold, iron and coal, she will be unable to buy one-thousandth part of the food which she has, during hundreds of years, thrown recklessly away.'"

Another possibility lies before us to avoid this position. So far we have never sought to utilize sea-plants as manure, except on a very minor scale. When one recalls the vast beds of weed in the Sargasso Sea, for instance, it seems probable that this immense amount of nitrogenous matter will not for ever be allowed to escape utilization in some way or other. These suggestions, however, are matters of the future. In the mean time, chemistry has foreseen this coming shortage of nitrogen, and has already begun to prepare for stepping into the breach when the Chili beds become exhausted. We may now describe the way in which this has been accomplished, for the problem is a very good illustration of the limitations within which chemistry has to move when it works on a large scale.

Let us see what is required. We need a product which in the first place contains nitrogen combined with other elements: this product must be suitable for manure, or in other words, either it must be a substance which the plant can utilize without any great trouble or it must be capable of decomposition which will result in products having this capacity. Secondly, our synthetic manure must be fairly compact in proportion to the amount of nitrogen which it contains, as otherwise the cost of carriage would be too great, and the problem of transport would be

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complicated. Thirdly, the cost of its production must be such as to enable it to be sold at a reasonable price.

Now let us turn to the present-day solution. In the first place, since the compound is to contain nitrogen, we must look for a cheap source of this element. Compounds are of little value to us, as they themselves without any further manufacture might be employed as manure. Evidently we must start with the element nitrogen itself, and build up a compound from it. Now we look round for a source of nitrogen, and it is clear that the simplest plan is to take the nitrogen which forms four-fifths of the atmosphere; it will cost us nothing and will be at the doors of any factory we choose to set up, no matter where we plant it. Having thus found our source of nitrogen, we turn to the question of cost. Obviously our process, in its final form, must be inexpensive; and since all work requires energy we have to seek for cheap power. With this end in view, the factories for this "fixation of atmospheric nitrogen," as it is called, have been set up in Norway or at Niagara Falls, in both of which regions water-power is cheap and plentiful. The next point we have to examine is the type of our artificial manure. Here we enter a region where only experiment can help us; it is not possible to say at once what particular nitrogen derivative will be of most value, for we have to consider also whether we can make it economically. A vast amount of investigation had to be made before this section of the problem could be solved, but we have now, actually working, four different processes which yield as end-products respectively nitrous and nitric acids, ammonia, cyanides and nitrides.

It is unnecessary to enter here into a description of all these types of manufacture; an account of the Birkeland and Eyde process as it is worked at Nottoden in Norway will be sufficient. We have already seen that the cheapest source of nitrogen is the atmosphere; but we have to

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combine this nitrogen with other elements before it will be suitable for manurial purposes. We thus come to inquire what elements we can utilize; and, as before, we must take cost into consideration. Obviously oxygen, which forms about a fifth of the atmosphere, is the cheapest element we could choose. This furnishes us with two elements. We have then to find some method by which they can be forced into combination. Now chemically speaking, nitrogen is an inert element; it will not readily combine with other elements, and under ordinary conditions it is without action upon them. Evidently we must find the conditions under which we can force oxygen and nitrogen to unite with each other. It has been observed that at very high temperatures, these two elements do combine; so that we have next to get some means of producing a high temperature. This has been found in the electric arc. Apparently all that is necessary is to burn an electric arc in air and the oxygen and nitrogen of the air will be forced to combine with one another. So much can be said on theoretical grounds; but when the matter is tested in practice, the electric arc is found to be rather a double-edged weapon: true enough, it does make oxygen unite with nitrogen, but at the same time its heat tends to decompose the compound which it has just formed. A compound of oxygen and nitrogen which is perfectly stable at ordinary temperatures becomes very easily broken down in the flame of the arc. Thus matters appear to reach a dead-lock. Further reflection, however, indicates a way out of the difficulty. The compound of oxygen and nitrogen is stable at ordinary temperatures; it is unstable only at high temperatures. Clearly, then, if the oxygen and nitrogen could be combined in the arc and the new compound which is formed were suddenly cooled it would be perfectly stable and suitable to our use. At first sight this appears to be a very difficult problem; but its solution

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is really simple. It is only necessary to blow a rapid jet of air across the arc. In its passage through the flame, the compound of oxygen and nitrogen is formed; this body is then carried along by the incoming air and forced out of the hot region before it has time to decompose. It flows into a much cooler part of the apparatus where it can be collected.

So much for the principle of the Birkeland-Eyde process. Its experimental details are rather complex, but there is one ingenious point which may be mentioned. It is a well-known fact that an electric arc is sensitive to magnetic effects. If a strong electro-magnet be fixed opposite the centre of an arc, the arc will be diverted from its usual position and will finally break; it will then reform at the electrodes, be diverted and broken again. In this way the simple arc can be replaced by a series of arcs which form, bend outwards, and break. If the apparatus be properly designed, it is possible in this way to produce an arc in the form of a thin disc instead of the usual narrow stream. The advantages of such an arc for our present purpose are twofold: in the first place, since the disc of the arc is very thin, a gas passing through it is heated only for a brief interval; secondly, owing to the increase in diameter of the arc it is possible, in a given time, to pass through it a much larger quantity of gas than we could pass across an ordinary narrow arc.

As to the other methods of nitrogen fixation, we need not enter into details. The electric furnace plays a preponderating part in them; but their rationale is rather too technical to lend itself to description here.

Before leaving this question of the nitrogen cycle in nature, we may point out a curious instance which shows how chemical problems may lie at the root of great economic changes. It is common knowledge that in recent years there has been a considerable migration from the

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United States into Canada; but to the man in the street the last place to look for the explanation of such a migration would be the chemical laboratory. Yet this emigration from the States is occasioned to a great extent by the fact that owing to the wasteful manner in which cultivation has been carried on there the soil has been deprived of its requisite amount of nitrogen and is consequently unable to produce the crops which it once did; while on the other side of the Canadian border lie vast virgin fields, stored with nitrogenous material and suitable for cultivation. This purely chemical point lies at the root of the great transference of population which we see going on at the present time; and it should serve as a warning lest in their turn the Canadian stretches are ruined in a similar manner.

In the foregoing paragraphs we have seen how chemistry is assisting us in the enlargement of our food supply; and we must now briefly discuss another section of the subject, although the progress in this direction is at present merely tentative. The second division of the food problem is the discovery of new supplies in addition to the sources which we are utilizing at present.

Experience has shown that the main foods of an animal under normal conditions can be divided into two classes: carbohydrates and proteins. By these names the chemist designates, in the first class, substances such as sugar and starch containing only carbon, hydrogen, and oxygen; while the second group consists of nitrogen derivatives. Further investigation proved that if animals were fed exclusively on sugar or starch, they were unable to live; while proteins alone, without sugar, were sufficient to support the vital functions. The problem of the proteins is, therefore, of some importance to us if we are ever to look forward to synthetic foods. Curiously enough, we owe the greater part of our knowledge of both sugars and

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proteins to the same investigator, Emil Fischer, whose work in these two departments is one of the greatest monuments of modern organic chemistry.

The importance of the proteins, moreover, is of interest, not merely from the point of view of a possible synthesis of edible material in the laboratory, it is also of great interest from the standpoint of biological chemistry ; for these proteins are the foundations of all organized tissue, whether it be animal or vegetable. Fischer, in the course of his investigations, has thrown a flood of light upon the chemical structure of this corner-stone of the living tissues, and in his laboratory he has synthesized many substances which are closely akin to the natural proteins. These researches, however, have been conducted in the face of almost overwhelming difficulties: for the practical side of them has proved a task which is within the competence of only the finest manipulators ; and, up to the present, the small amount of material produced is out of all apparent proportion to the labour expended, so difficult is the work. The cost, also, of preparing these "pseudo-proteins" has been enormous. Nevertheless, now that the foundation of our knowledge is securely laid, it is within the limits of possibility that in the future the advances may be rapid. We have seen how the electric furnace has developed from being a scientific curiosity into one of the most potent adjuncts to industry ; and it is too soon to despair of the synthetic preparation of food-stuffs, remote though that possibility may appear. Even at the present day we see what may be the beginning of a new development. We know that numerous artificial flavourings are at present in common use ; margarine, obtained from animal fats or vegetable oils, is competing in our markets with the cheaper grades of butter ; saccharin is the laboratory substitute for sugar, over which it has advantages in the case of certain diseases ; while a very large quantity

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of grape sugar is really manufactured in the laboratory and not extracted from grapes at all. With these examples before us, it seems rash to assert that chemistry will make no further strides in the direction of synthetic food.

But even assuming that such a consummation is beyond our powers, it must be admitted that chemistry is every day stretching out into new fields which touch most closely the root-problems of our existence. It is increasing our present sources of supply, and with the coming of agricultural chemistry we are approaching a stage where the materials we possess can be put to better uses than has been the case in the past.

We have now seen how chemistry is being applied to the problem of the food supply and to the question of power, but in recent times an even more vital riddle has been proposed to it: nothing less than the synthetic production of living tissue. Let us see how we stand with regard to this.

A living creature, as every one will allow, has a material basis in which certain changes are continually taking place; from this it follows that the problem of the artificial production of living protoplasm entails two minor questions. In the first place, it will be necessary to produce synthetically the physical and chemical basis corresponding to the ordinary animal's protoplasm; and, secondly, it will be essential to reproduce in this material certain chemical changes which we regard as the physical manifestation of life.

The researches of Emil Fischer have shown that it is quite within the bounds of possibility to produce chemical compounds which will be akin in almost every respect to the albuminous bodies from which the major part of living beings is built up. We already have in our laboratories the means of synthesizing such materials, and further advances are to a great extent merely a question of time and money. So far, we are on safe ground. But when we come to the problem of the chemical reactions by which

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life manifests itself, we have to confess that as far as we have gone we have found very little sign of such changes being set up in the synthetic proteins; we have not even any very clear ideas as to the methods by which such reactions could be brought into play. In fact, the present position might briefly be stated as follows. We have the means of building up more and more complex protein derivatives, and, sooner or later, we shall probably synthesize substances quite as complex as the natural protoplasmic materials; when this point is reached, unless our knowledge of "vital" reactions has considerably advanced, we shall at best be in the position of a watch-maker who has constructed a watch but has forgotten to make any contrivance for winding it up. At this point, chance might enter into the problem, and the protoplasmic machine we have designed might spontaneously set itself in motion, but more than this we are not entitled to expect. Experiment is the only possible test, and the date of the crucial trial is still far distant.

Then there are other difficulties in the way, even assuming that the protoplasm became active without assistance. Let us suppose that in our synthetic compound some small speck became alive. Unless it were able to react upon the rest of the substance and set it in action also, we should very probably never detect the vitality of the particle which had become living. Things are not so simple as they might appear, and no one imagines that a mass of synthetic protein will transform itself into, say, a jelly-fish overnight. Looking back through the geological periods, we see the extremely slow development of type after type among the living kingdoms; the earlier creatures are beyond our ken, for their flaccid materials have left no fossil traces behind them; but it seems more than probable that they in their turn suffered a long age of development during which mere microscopic specks grew into creatures

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like our present-day amœba. Those extremely rudimentary particles of vitalized material which came into existence in the earlier days of the cooling earth may have differed entirely in their type from the living things which we know; and we might be hard put to it, were we confronted with them, to decide whether they were actually alive or were mere conglomerations of chemicals decomposing in a somewhat complicated manner. But this is very probably what synthetic living tissue would resemble, were we able to produce it to-day. However, this question is distinctly one of the future and not of the present, and we can safely leave the synthetic chemist of that day to apply the appropriate tests as soon as he secures something to experiment with.

Purely as a matter of speculation, however, one cannot help dwelling on the possibilities before such a discovery. Usually, it is assumed that it would be of purely theological interest, a good weapon for the materialist, and so forth. But it might very well turn out to be of considerable mundane importance. Suppose that this new protoplasm had properties slightly different from those types which we know; its accidental discovery might involve us in very serious consequences. Assume that it had great powers of assimilation and reproduction, and we might find it rather a dangerous neighbour, so that finally the new discovery might end in the rapid extirpation of the long-sought-for product. This is the grotesque side of the matter. Even more serious, however, would be the state of things if the synthetic creature resembled our ordinary bacteria, and was capable of lodging in animals, and there liberating new forms of toxins against which we are not immunized. It is just a possibility, but it would certainly be a most awkward end to an experiment. Such speculations, however, take us out of the chemical field entirely, and belong in the meantime to pseudo-science.

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CHAPTER XIV

THE METHODS OF CHEMICAL RESEARCH

THE main object of any scientific investigation is, of course, the attainment of a more complete knowledge of some section of the subject; but there are two possible ramifications of original research. In the first place, the investigator may be seeking to discover facts which, though of extremely important character from the point of view of theoretical principles, are of no apparent direct practical applicability; or, in the second place, he may be carrying out what is termed applied research, in which he is utilizing the knowledge previously acquired for theoretical ends, and applying it to the solution of practical problems. Thus we have a rough distinction between the two types of investigation: pure research and applied research.

In the previous chapters of this volume several examples of pure research have been dealt with at some length. Ramsay's investigations in connection with the rare gases, and the major part of the researches on radium which have been described, will suffice to give a general idea of this class of work in its main outlines. In the present chapter an attempt will be made to throw some light upon the actual methods employed in chemical investigations, and also to treat the question of applied research so as to bring out the factors which govern such lines of investigation.

When a chemist is asked to undertake any investiga-

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tion, the first problem which presents itself to him is that of securing perfectly pure materials. It is of very little use carrying out the most refined manipulations if the substances dealt with are in an impure condition, for the results obtained at the end of all the work would be vitiated by the presence of the extraneous material. Evidently, then, one of the most important features of an investigation is the preparation and purification of the materials which are to be used in the course of the research.

There are three chief methods by means of which a substance can be separated from its impurities: distillation, crystallization and precipitation. Let us take these in turn.

Suppose that it was necessary to purify some sea-water so as to obtain a specimen of uncontaminated water. The sea-water is placed in a flask and brought to the boiling-point by the aid of a burner. The steam which escapes from it is passed through a condenser—a glass tube surrounded by a second tube through which cold water is allowed to flow—with the result that the steam is condensed to water and can be collected. At the temperature of boiling water, the salts which sea-water contains are not volatile; so they do not vaporize, but remain unchanged in the flask. In this way it is possible by a process of distillation to separate the salts from the water. The same process may be applied even when the impurity is a volatile one. Distillation is carried out as before; but the process is broken off when, say, half the total volume of liquid has passed over into the condenser. Now imagine that the impurity is more volatile than the substance we wish to free from it. It is evident that the impurity, on account of its high volatility, will vaporize first, so that the half of the liquid which has passed over into the condenser will be richer in impurity than our original material was, while the substance left behind in the flask will contain

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less impurity than the original material. By repeatedly distilling the second fraction and rejecting the first portions which pass over, it is possible gradually to get rid of all the impurity, and a pure specimen of the material which was required is left behind.

Take next the method of crystallization. A simple experiment which can be performed by any one will serve to illustrate the uses of this process. To a vessel of boiling water sugar is added until a saturated solution is obtained ; then a pinch of salt is dropped in. Clearly there results from this a solution of sugar with a trace of impurity in the form of salt. Now, sugar is much more soluble in hot water than in cold; so that as the vessel cools down some of the sugar will come out of the solution and form crystals. This is because so much sugar has been added to the water that what is termed a supersaturated solution at the ordinary temperature has been formed. But with the salt it is different. The pinch of salt which was added to the water would have dissolved quite readily in the same quantity of cold water ; so that when the solution cools to the ordinary temperature the salt will still remain dissolved in the water. Thus as the liquid cools down a steady separation of sugar will go on, but no salt will come out of solution. All that is necessary, then, is to wait until no more sugar separates ; pour off the super-natant liquid, and then wash the sugar crystals with a very little pure water to remove any traces of salt water from their faces. The crystals, after this treatment, are free from salt.

The third method of purification with which we need deal, precipitation, can be understood from the following example. Camphor and potassium iodide are both soluble in alcohol, but while potassium iodide is soluble in water, camphor is practically insoluble in this medium. If then, we were given a solution of potassium iodide and camphor

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in alcohol and were asked to separate them, all we require to do is to add a large quantity of water to the solution. The potassium iodide would be easily soluble in the water; but the camphor would be unable to dissolve, and as we added more and more water the alcohol would become so diluted that it would be unable to retain the camphor in solution. Finally, we should find the camphor precipitated from the liquid as a powder, which we could collect, wash, and regard as pure. Another example is more complicated. Our silver coinage contains a mixture of silver and copper. Suppose that we were asked to obtain the silver in a shilling free from the admixed copper. In the first place we should try to dissolve the coin, and we should find that the best solvent was nitric acid. The whole shilling would go into a solution and we should get a blue liquid which would contain two salts, nitrate of silver and nitrate of copper. These might be separated by crystallization in the usual manner, but there is a speedier and more efficacious method available. It has been found that chloride of silver is insoluble in water while chloride of copper is easily soluble. Obviously, then, if we could convert the two nitrates into the corresponding chlorides we should find the silver chloride insoluble while the copper chloride would remain in solution. Hydrochloric acid is added to the solution, silver chloride is formed and falls out of the solution in the form of a white flocculent precipitate, while the copper nitrate is changed to copper chloride but remains in the solution. We have thus separated the silver of the shilling from the copper. All we need do to obtain the silver is to collect the white silver chloride, dry it, and heat it to a high temperature with charcoal. The result is the liberation of the metallic silver which we require.

When a substance has been passed through these processes of purification, it is necessary to apply further tests

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to ensure that it really is free from any admixture of other bodies. There are various methods by means of which this question may be determined. The physical properties of a pure substance are always constant: for example, water under ordinary atmospheric conditions always boils at 100° C., and ice melts into water at 0° C. Thus if the physical properties of the pure substance are known it is possible to determine directly whether a given specimen is pure or not. If, however, the substance is a new one whose properties have not been investigated, a more laborious process must be gone through. The physical properties of the body are examined and tabulated; it is then subjected to repeated purification operations, and the physical constants are examined after each of these stages. If no change is noted in the physical properties, evidently the purification is complete, as no impurity is being removed.

Sometimes, however, it is not possible to take the physical properties of substances as a guide; and recourse is then had to analysis. Suppose that a specimen of sodium chloride has to be tested for purity. It is known that when sodium chloride (which is a compound of one atom of chlorine with one atom of sodium) is allowed to interact with silver nitrate in solution, a white precipitate of silver chloride is thrown down owing to the silver attracting the chlorine away from the sodium. Now, the amount of silver in the silver chloride can be found by converting the chloride into metallic silver, and hence the amount of chlorine in the chloride can be estimated, for it will represent the difference in weight between the silver and the silver chloride. But this chlorine was contained in the amount of sodium chloride which was taken in the first experiment. The amount of sodium in the sodium chloride can be estimated by means of other chemical reactions; and if the weight of sodium thus

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obtained be added to the weight of chlorine previously determined, the two together ought to be equivalent to the total weight of sodium chloride employed. If the numbers add up to less than a hundred per cent. then clearly some other substance must have been mixed with the sodium chloride.

Another example of analytical methods may be given, as it shows the methods of avoiding experimental difficulties which are utilized in the chemical field. A constituent of turpentine is composed of carbon and hydrogen; and it may be necessary to analyze this compound. We have no direct method of estimating either carbon or hydrogen, and at first sight it appears as if the problem would present some difficulties. Suppose, however, that we allow the turpentine gas to burn in oxygen, we find that the carbon contained in it is oxidized to carbonic acid gas, while the hydrogen combines with the oxygen to form water. This gives us the essentials of a method. The major details of the experimental part are as follows. The turpentine derivative is burned in an enclosed vessel by means of injected oxygen, and the resulting carbonic acid gas and water are blown into receivers. Both the receivers are weighed before the beginning of the experiment; one of them is filled with concentrated sulphuric acid, which has the power of trapping water; while the second receiver contains strong caustic potash, which combines with carbonic acid gas. After the experiment is over, the two receivers are again weighed. The increase in weight of the one represents the weight of water produced in the course of the experiment, while the gain in weight of the other corresponds to the weight of carbonic acid gas liberated. Now, water contains one-ninth of its weight of hydrogen, while carbonic acid gas contains three-elevenths of its weight of carbon. A simple calculation gives us the amounts of carbon and hydrogen liberated from the turpentine derivative, and, knowing the weight of

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this body which was used in the experiment, it is a simple matter to determine whether our specimen was pure or not.

Another problem which often has to be solved is the discovery of what is termed the molecular weight of the substance under examination. It is found that hydrogen gas in the free state never contains less than two atoms of hydrogen; we never find single atoms of hydrogen in existence. Two atoms of hydrogen united with one another are said to form a molecule of hydrogen; and the molecular weight of hydrogen is said to be 2; that is, it is equal to the weight of two hydrogen atoms. Now, according to Avogadro's Law, if two substances exist in a state of vapour, equal volumes of the two vapours will contain equal numbers of molecules. Thus, if we take a litre of hydrogen and a litre of acetylene gas, each litre will contain the same number of molecules. To determine the molecular weight of the acetylene gas, then, it is only necessary to weigh a litre of it and compare the weight so obtained with the weight of a litre of hydrogen. Then, since the molecular weight of hydrogen is 2, the weight of the acetylene gas divided by 2 will represent the number of times an acetylene gas molecule is heavier than a molecule of hydrogen. The following figures will make the point clear:—

1 gramme of hydrogen occupies 11·1 litres
13 grammes of acetylene occupies 11·1 litres
} Same number of molecules.

Acetylene is therefore 13 times heavier than hydrogen. But the molecular weight of hydrogen is 2, so the molecular weight of acetylene must be 13 times this, or 26.

The molecular weight is of use in telling us how many atoms a compound contains. The sodium atom is twenty-three times heavier than the hydrogen atom, while

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the chlorine atom is thirty-five and a half times as heavy as the atom of hydrogen. Suppose that the molecule of sodium chloride contained two sodium and two chlorine atoms, clearly it would have a weight equal to $2 \times 23 + 2 \times 35{\cdot}5 = 117$. In actual practice, however, it is found to be half this, viz. 58·5. This figure would correspond to the weight of one sodium atom (23) plus that of one chlorine atom (35·5), so it becomes clear that the molecule of sodium chloride contains only one sodium atom and one chlorine atom.

We now come to the question of determining the chemical constitution of substances; or, in other words, the problem of the manner in which the atoms are linked together within the molecule. The methods employed are too technical to lend themselves to simple description, but the principles upon which they are founded are not difficult to grasp. Suppose that, by acting upon a substance with a certain chemical, we find that a group of atoms is removed from the first body, we shall be inclined to suspect that these two atoms are in some way more closely connected with one another than with the rest of the compound. If further investigation shows us that this group of atoms can be transferred from compound to compound *en bloc*, and that each compound acquiring them gains a new set of properties, which are lost when the group of atoms is not present; then we are entitled to say that these atoms are evidently closely related to one another, and that the property which is present when they are present and absent in their absence, is a property inherent in this particular grouping of atoms. This is the basis upon which our ideas of chemical structure are raised.

In the domain of organic chemistry, our views are now so firmly rooted that it is possible for a chemist to write down a formula for some body which has never been prepared; and, founding his work upon past experience of the different

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groupings of atoms expressed in the formula, to proceed in his laboratory to the synthesis of this body. Many of the naturally occurring substances, such as camphor, have been prepared in this manner; the chemical constitution of the substance was deduced from the reactions which it showed, and from this evidence a symbolical formula was suggested. A substance corresponding to this formula was then built up, and on examination it was found to be identical with the natural product. It must not he supposed, however, that the process so simply described is an easy one; the experimental difficulties in the way are often exceptionally great, and it may require many years of work before they can successfully be surmounted.

Let us now turn to consider the other branch of chemical investigation: applied research. In this division of the subject there are two great branches: for the applied chemist may turn his attention either to the combination of known processes, in order to produce a new method; or he may take a process already in existence, and seek to improve it in detail, while leaving the principle intact.

In order to illustrate the methods of applied research, it will be well to take a concrete example. The saying is attributed to Lord Beaconsfield that the commercial prosperity of this country could well be gauged by the condition of its chemical trade, and as the alkali industry is one of the greatest branches of our commerce, it will serve as a typical example for our present purpose.

The history of the alkali industry is a very chequered one. If we go back to the eighteenth century, we find that carbonate of soda, as an article of commerce, was obtained from various natural sources. The chief supplies were obtained from kelp, the residues of sea-plants; from barilla, the ashes of land-plants from Spain; from latroni, a saline efflorescence found in Egypt and on the shores of certain salt lakes; and from common salt, by a very crude pro-

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cess depending upon the addition to it of the ashes of land-plants.

The matter might have remained at this stage for a considerable period, but the coming of the French Revolutionary wars brought a new factor into the problem. Sodium carbonate, as we shall see later, is an important chemical in several industries. Now, the progress of hostilities had interrupted the importation of external supplies of the carbonate into France, and, as a result, the group of industries connected with it had suffered very considerably. The French Government was forced to appoint a Commission to investigate the question of opening up new sources of supply, and this Commission, in 1794, reported favourably upon a process devised by an apothecary named Leblanc. The Leblanc process, as it is termed, took as a starting material common salt, *i.e.* sodium chloride, to give it the chemical name. This body could, of course, be obtained in any required quantity from the sea. Common salt was first treated with sulphuric acid, which converted it into sodium sulphate, usually termed "Glauber's salt," from the name of its discoverer. The "salt-cake" or sodium sulphate was then decomposed with coal and chalk at a red heat, and the residue was found to contain carbonate of soda, which could be dissolved out from the charred mass by means of water.

Leblanc, however, did not reap the fruits of his discovery. Owing to the disturbances of the Revolution and the ensuing war, he lost his funds ; and, in consequence, his works were closed, and the process came to a standstill. The founder of a world-wide industry received no reward from his Government, despite its promises, and in 1806 he committed suicide in an asylum.

The failure of Leblanc's works, however, did not deter others from taking up the industry, and several manufactories sprang up in France in which the Leblanc process

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was utilized. It was not until almost a generation had elapsed, however, that the trade was brought into active existence in England. In 1814 an attempt was made to set it going; but at that time a duty of £30 per ton on salt existed in this country, which militated against the working of the process. In 1823 this duty was repealed, and in the same year Muspratt opened his alkali works in the vicinity of Liverpool.

We now come to a point which illustrates the problems with which the applied chemist must deal. In the original Leblanc process, as worked in France, sulphuric acid was obtained from sulphur, since this substance was easily obtainable from the deposits in Sicily. Shortly after the founding of the industry in this country, however, the Sicilian sulphur mines became a monopoly; and, consequently, after 1839 the alkali manufacturers had to secure a new source of sulphur from which to prepare their sulphuric acid.

It was known that the iron pyrites could be utilized as a source of sulphur, but the process of extraction did not at first sight appear to be a commercially paying one. Investigation showed, however, that pyrites contain a certain proportion of copper, amounting to perhaps two per cent. If this copper could be extracted, its value, in addition to that of the sulphur, would make the treatment of pyrites a remunerative industry. In 1865 a method was discovered by means of which the copper could be obtained from the pyrites, and, in consequence of this, it became possible to utilize the pyrites as a source of sulphur. Further, the extraction of the copper led to the foundation of a new industry, and the two trades of copper extraction and alkali manufacture became very closely interwoven.

Another branch in the industry next came to life. In the process of converting common salt into sodium sulphate,

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a vast quantity of hydrochloric acid gas was liberated, and in the earlier stages of the industry this gas was simply allowed to escape into the air, where it worked havoc with all vegetable life within miles of the works. This led to the rise of very bitter feeling against the trade, and it became evident that some improvement must be effected. Again we find a close connection between political events and industrial processes. Previous to 1837, the duty upon paper had been 3*d.* per pound, but this was subsequently reduced to 1½*d.*, and in 1861 the paper duty was abolished. As a consequence of this cheapening of paper, a much greater use was made of the substance, and naturally the trade in materials for paper-making grew in importance. Now, in the manufacture of paper it is necessary to employ bleaching agents, one of the most important being chlorine, which is a constituent of hydrochloric acid gas. It thus became possible, instead of wasting the hydrochloric gas of the alkali works, to utilize it in the paper industry ; and the industrial chemist was not slow to produce a method by the aid of which this utilization became possible. Thus, partly by political changes, and partly by the ingenuity of the applied chemist, the alkali industry was now able to employ at a profit a substance which previously it had regarded as a mere waste-product ; and it was also in a position to cause less nuisance in the neighbourhood of its factories. Since 1863 it has been illegal to allow the escape of noxious gases from alkali works.

Thus even at this early stage in the alkali industry three other trades had grouped themselves about it: the extraction of copper from pyrites, the manufacture of sulphuric acid, and the production of bleaching-powder. In the seventies of last century, the Leblanc process seemed to be holding the field against all comers, and to be in a perfectly stable position. Yet applied research, which had been the making of it, was destined to produce

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in time a rival which was to bring the Leblanc process into a period of stress such as it had never encountered.

In 1838 a process was patented which was destined to compete with the long-established industry. If a solution containing common salt and ammonia be saturated with carbonic acid gas, it is found that the common salt is converted into bicarbonate of soda (baking soda). Although when expressed in this manner the process appears simple, it is in practice surrounded by very great technical difficulties; and on its first appearance it seemed as if the ammonia-soda process, as it is called, might not be a commercial success. It was tried in France in 1855, but after two years the factory was abandoned as the process did not appear likely to be commercially successful. In 1861, Solvay erected a factory near Brussels, and after five years spent in coping with apparently insuperable difficulties he was enabled to produce an apparatus which seemed to promise paying results. In 1872 he erected another factory near Nancy, and was able to turn out ten tons of soda per day; in 1873 it began to be recognized that the Leblanc process had met a dangerous rival, and, in 1874, the process was started in England by Mond and Co. (now Messrs. Brunner, Mond and Co.). In 1895 the two processes were almost neck-and-neck, but the ammonia-soda process had won: 428,614 tons of soda were manufactured in this way, while the Leblanc process was responsible for 408,107 tons. In 1900, the ammonia-soda production amounted to 900,000 tons, while the whole world-production by all processes may be placed at 1,500,000 tons. In 1904 the British works utilizing the ammonia-soda process consumed no less than 1,703,805 tons of salt.

One factor in the problem has told in favour of the Leblanc process. It will be recalled that in this method the chlorine is utilized in the form of a bleaching agent. In the ammonia-soda process, however, this element is lost

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as far as commercial extraction is concerned; so that here we have an example of the extreme importance of finding some use for by-products in our factories. Were it not for this factor, which in the earlier days of the industry was allowed to escape without any thought, the Leblanc process would not be in a position to compete successfully with its younger rival. The competition forced the workers of the Leblanc process to turn their attention to the recovery of other by-products; and means were taken to recover the sulphur from the alkali waste of the factories by chemical methods, so that a further saving in cost was obtained in this manner.

About 1890, it became evident that the competition between the two processes was becoming reciprocally hurtful; and forty-four of the leading Leblanc manufacturers entered into a combination with a view to bargaining with the ammonia-soda factories; and from this arose one of our greatest trade combinations, the United Alkali Company.

We have now given sufficient details to show the factors which are predominant in applied research work. In the first place, the starting-materials for any process must be cheap and also easily obtainable; secondly, the cost of manufacture must be kept down by simplifying the process as far as possible; third, every by-product must be scrutinized and if possible utilized in other trades. It is only by a careful survey of every possible source of profit that any industry can hope to compete successfully with the rivals which further research is sure to rear up against it.

Now let us turn to the ramification of the alkali industry through other trades. We have already had occasion to notice the close connection between the bleaching-powder and alkali industries, and the manner in which the copper trade has been associated with the fortunes of the Leblanc

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process. But there are other trades which nowadays are also in direct union with the alkali industry. Prominent among these stands the manufacture of soap. Tallow and many other substances belong to a class of substances known as ethereal salts; they are produced by the combination of organic acids with glycerine or some similar body. If, now, we replace the glycerine by some alkali, we get the metallic salt of the organic acid; and these metallic salts are what we term soaps. The nature of the soap produced will depend upon the particular acid and alkali which we choose; so that it is possible to manufacture different kinds 'of soap each of which will have its appropriate use. Thus the products of our alkali factories are to a great extent the raw material of our soap factories. On the other side, the soap industry touches the glycerine trade; for glycerine is a by-product of the soap-works. The alkali industry also impinges upon glass manufacture; though in recent years sodium sulphate has been more extensively utilized in the making of glass. Ultramarine is also a material which owes much to the products of the alkali trade.

Thus it becomes clear that as soon as applied chemistry has opened up one line of manufacture, other industries will develop as offshoots and bring their problems in turn to the investigator for his solution, until it becomes almost impossible to trace the original trade through all its associations. Enough has now been said to show the type of problems which face the applied chemist and the importance of the work upon which he is engaged.

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CHAPTER XV

THE ORGANIZATION OF CHEMICAL RESEARCH

IN the previous chapter we have seen some of the methods employed in chemical investigation; and have gained certain ideas with regard to the technique of the subject. But this survey has taken into account only a portion of the problem, for it leaves out of account the human factor, which is decisive in research as it is in most other lines. Let us now turn to this part of the research field and endeavour to gain some conception of how we can get the best out of our human material.

In the first place, we must glance at types of research which are being conducted in our laboratories, for until we know the problems to be attacked we can have little idea of how they can best be solved. When we look round at the general trend of chemical investigation, we find that there are two types of researches, speaking in broad terms. There is the kind of research which can best be conducted by one or two men; and there is another class of investigation which necessitates the employment of a large number of collaborators. Let us see how such a distinction arises.

Suppose that a subject has to be dealt with whose treatment entails extremely refined manipulation; it is evident that only a few men in the whole world are capable of coping with such a task, for the aid of inferior workers would not help matters in the least. Such subjects,

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obviously, must be handled by a very select minority of investigators to the exclusion of less well-trained workers. Again, the quantity of the substance under investigation may be limited; and here again it will only be possible for a very few investigators to conduct researches upon it. The earlier stages of the radium investigations furnish an example in which both those factors were combined; for on the one hand the technique of the work was far beyond the powers of any but a few researchers; while on the other side the quantities of material available for examination were very minute.

The second type of research is conducted under conditions diametrically opposed to these. It is often necessary to amass an immense collection of data before any conclusions of value can be drawn from them. Now, these data in turn entail for their ascertainment a vast amount of experimental investigation; and in many cases it is impossible for any one man to carry out all the practical work which is required; even a lifetime might not be sufficient for the purpose. Thus it becomes essential to enlist the aid of many collaborators in the work. When these workers are grouped round some outstanding investigator, we have the foundation of what is termed a "school of research."

From the nature of chemistry, the collaborative method is the favourite one, especially in Germany. There, it is usual to find a man who has come into the front rank of science supported by a number of lesser investigators who are ready to place their brains and manipulative dexterity at his disposal in the furtherance of some extensive piece of research. In France, on the other hand, single-handed investigations are more common; while in our own country the two types are not seldom found side by side.

It is never safe to dogmatize in such matters, but from a practical point of view it appears probable that the co-ordinated system of research will yield, on the average, a

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better result with the material which our technical schools and universities are producing. It must be borne in mind that we cannot arrange our conditions entirely with a view to the attainment of the best results if a scientific genius happens to come upon the scene at any moment; such a person only appears at rare intervals in the history of a science, and to plan our research organization solely with the idea of utilizing him to the best advantage when he does come would be to militate against the success of men who, though they are not geniuses, may yet be far above the ordinary intellectual level of their fellows. A man of the type of Pasteur, when he comes into science, is not hampered by any system which may be in existence; he is capable of utilizing it to the utmost, and of superadding any details which may be necessary for the furtherance of his work.

The career of a successful investigator may, generally speaking, be divided into four sections. First we find him in his student days acquiring the outlines of the science as a whole and storing up a mass of information which may at a later date help him to a true perspective of the field in which he decides to work. Then there springs up in him the desire to advance his subject, to do something which is not already recorded in the literature, and to find out for himself how far the statements which he has read are of universal application. At this point he is generally taken in hand by one of his seniors, and the second stage in his career begins. He is not usually fit at first to undertake an unaided investigation; but he is capable of assisting a more advanced worker; and during this term of apprenticeship he learns the difference between book-knowledge and actual practice. He finds that what on paper seems to be a simple operation really involves immense labour; and he begins to realize how far it is safe to push an inference without running the risk of overshooting the

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truth. When he has passed through this mill, he has acquired sufficient grip of his subject to admit of his taking up a line of his own, and so he enters the third stage as an investigator. He has now behind him enough work to allow him to feel that he can stand alone; he has gained an experience of methods; and he has acquired a sense of perspective which will prevent him from confusing details with the main principles of the question which he attacks. This period of his scientific life may be long or short, according to his natural ability; if he has little originality, he may never advance beyond this sphere; but if he is a person of exceptional insight he will turn his attention sooner or later to problems of greater and greater importance, until at last in the final phase of his career he may be reckoned among those whose investigations have revolutionized our views in certain fields.

It will be seen that his career can be looked at from another point of view: during the earlier stages, until he reaches the stage of independent research, he is receiving the stimulus of his teachers and colleagues; while in the later stages he will be able to repay this debt by assisting those junior to him with his experience and advice.

It is noticeable in the works of at least one author who quitted his scientific work at a fairly elementary stage, that a certain tendency to scoff at the idea of "training in research" makes itself apparent. No one, of course, suggests that by putting an average man through any imaginable course of instruction we shall produce a Pasteur, a van 't Hoff or a Ramsay; such an idea is too absurd to be worth discussing. But there is no doubt, in the mind of any one who has been closely in touch with research work and who has actually carried out a reasonable number of investigations of his own, that a student can acquire a grasp of research methods much more rapidly and with fewer mistakes if he conducts his first experiments with a more

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experienced investigator at his back to suggest simpler methods and more rigid tests than a beginner would be likely to think of for himself. This is all that the phrase "training in research" suggests to the scientific world, though apparently pseudo-scientific writers cannot get the idea of schoolmastering out of their heads.

There is another factor which bulks largely in the early training of an investigator, a factor which is so elusive that we can describe it only under the name of "atmosphere." The main object of all investigation is to acquire some knowledge which will, as far as possible, stand the most rigid inquiry into its correctness. Now, the human mind is peculiarly liable to work in a groove; and the scientific man's mind is not very different from any other in its essential failings. Thus it comes about that an investigator is very apt to look at things rather one-sidedly, and in consequence of this he may miss an important point which lies just beyond his purview. In a large laboratory, where he is surrounded by other workers, a researcher generally finds that his neighbours are only too glad to listen to his views and to criticize them freely; and in this way he may be headed off from false lines of deduction at an early stage. On the other hand, an isolated worker is not subject to this corrective until his work is published in its final form, and then it is too late to carry out the subsidiary investigations which might have thrown light upon disputable points. Again, in a large laboratory filled with investigators many of whom are working in different branches of the same subject, a man often finds that one of his colleagues, looking at his research from a different point of view, is able to bring a new light to bear upon it, and thus suggest fresh applications of the work which he himself would probably never have realized. This kind of assistance is invaluable to an investigator; for the help of other workers brings into the common stock a much more varied experience

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than the single handed worker can possibly have at his disposal.

We have now gained some idea of the outlines of the problem indicated in the title to this chapter; and we may proceed to develop the matter in more detail.

The ideal system of organizing research in chemistry can be sketched in a few paragraphs, and we may then compare the result with our present system in practice; the comparison will show us where the weak points of our everyday methods lie, and we can make an endeavour to find out whether these defects are beyond remedy or not. There is one point to which attention must be drawn at the outset. Research workers as a rule are not greedy for wealth; the average income of the ten greatest scientific investigators in this country would certainly not satisfy a successful business man, and the majority of scientific workers exist on salaries which would not make a middle-class shopkeeper envious. This is not in itself a great grievance, for the scientific worker has other compensations. But at the same time if the scientific man is to be poorly paid he should at least be assured of reasonably rapid promotion, especially in the junior grades; and this is the weak spot in the present system, as we shall find later. We have seen in earlier parts of this book, that applied science is founded upon the work of the pure investigator; and if the community is benefited by this work, as it undoubtedly is, it seems only fair that some slight return should be made for the value received.

Now let us take the case of a young man who feels that his interest lies in chemistry, and try to imagine how his development could best be fostered.

In the first place, he would enter one of the universities, university colleges, or technical schools. Here he would, for a term of three or four years, spend his entire time in acquiring a knowledge of the outlines of at least three and

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probably four sciences. He would also have to gain enough knowledge of German and French to permit him to read scientific papers in these languages; and if he had time, a smattering of Italian would be of advantage. During the later stages of this period of his training he would be brought into contact with investigators in different fields of science, and would probably meet men who had won a place in the front rank of scientific discovery.

When his knowledge of the broad outlines of chemistry was sufficient, he would be encouraged to take up some research work in conjunction with a young investigator; and, if his technical skill and mental ability fitted him for the work, he might be later employed as a private research assistant to one of the chiefs of the laboratory, so that he might feel that he was actually associated with chemistry in the making. During this period he might be given a scholarship of a value of, say, eighty or a hundred pounds per annum, which would enable him to live and buy the books which are essential to a scientific worker.

At the end of this stage in his career, he might look forward to a post as a teacher in his own or some other institution. Preferably he would be transferred to another university. Owing to his research work, he would have by now acquired a thorough knowledge of the history and present progress of the branch of chemistry in which he was working; and this might be utilized in the form of lectures to advanced students. He might also be entrusted with the supervision, under the control of some higher official, of the practical work of a section of a junior laboratory. It would, at this stage of his career, be inadvisable to let him lecture to junior students; for though in an advanced class the lecturer can take a considerable knowledge for granted on the part of his audience, junior teaching is a much more difficult problem, since nothing can be taken for granted. The salary to which he might be entitled at this time would

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probably be £120 to £150 a year; and for this, in addition to his teaching, he would be expected to continue his research work.

After a year of this, he would have acquired sufficient experience and self-reliance to make his further promotion desirable; and he might be given a Research Fellowship of, say, £150 per annum, which would enable him to go abroad and pursue his investigations in some of the Continental laboratories; the object of this being to give him an insight into other methods of organization and research. If his research record were deemed sufficient, this Fellowship might be extended to a term of three years.

By this time, he would probably be twenty-six or twenty-seven years of age, and on his return he might look for a post as Lecturer either in his own or another institution. Or, if his bent lay in that direction, he might decide to devote himself to applied chemistry, and enter the laboratory of some chemical manufactory. In either case, with his training and equipment he might reasonably look forward to a salary of about £250 per annum.

If he chose the university as his sphere, he might remain as a Lecturer for three or four years, say, until he was thirty; then, if his work was of sufficiently high quality, he might be promoted to an Assistant-Professorship with an annual salary of £350 or £400. In this stage he would have charge of a special department of chemistry in his university; would be able to call upon his students to assist him in his research work; and would generally be in training for the highest grade of his profession.

Finally, as soon as he showed himself capable of really important work, he might be given a Professorship in his chosen branch of the science, with a laboratory of his own and several assistants. His salary might at first run to £500 a year, with increments according to his merits. He would by this time have won a recognized place in chemistry,

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and his example would be of value to his juniors ; he would be able to attract students to his laboratory for the chance of working under his direction ; and he would have behind him a sufficient number of assistants to allow him to launch into such investigations as entail the collaboration of numerous workers.

In such an ideal system, a man on entering upon his career would know definitely that if he proved himself worthy of promotion he would get it ; whereas, if he failed, he would meet his deserts and could expect no further advancement. Compare with this the actual machinery which is in existence at the present day.

Let us take the case of just such a young man who feels a liking for chemistry and wishes to devote himself to the study of that subject. He goes to a university or some analogous institution, and spends three or four years in taking his science degree. During this time, he is much out of pocket ; for not only must he keep himself, but in addition he has to pay fees, which in some university laboratories are very high. For example, in one the fee for attendance at the chemical laboratory is twenty-five guineas per session, while the cost of materials used must be defrayed besides. Lectures, which may run to three or five guineas per course, have to be attended ; and this only covers one subject for his degree, the others being almost equally costly.

If he be fortunate, he may have secured a scholarship on entering the university ; but everybody cannot do this. Further, in the usual scholarship examinations of this type the tests applied are suitable for the brains of schoolboys, and it is by no means certain that the bright schoolboy will develop into a person of much value in later life ; the contrary is more often the case. Thus the people who get scholarships at this stage are not necessarily the most desirable as far as their future chemical training is concerned.

There may be other scholarships open to him later in

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his undergraduate career which are not subject to this objection, as they are awarded on his more mature knowledge of the special subject to which he proposes to devote himself; but here we come to the real trouble of our whole system, its lack of elasticity. Every university teacher knows that his students from year to year do not represent a dead level of merit; in some sessions he may have four or five men who are distinctly above the average, while at other times a couple of years may pass in which no candidate escapes the dead level of mediocrity. Nevertheless, most scholarships have to be awarded at fixed times, independently of whether the applicants are merely average in ability or are outstanding in their merit; with the result that if three exceptionally good men appear in one year two of them may get no assistance, while a much less efficient man may secure the scholarship on the following occasion of its award. It is evident from this that what we need in future is a system whereby there will be no necessity to award scholarships unless really good candidates make their appearance; whereas in other years the money thus saved may be devoted to encouraging a larger number of men who have greater claim upon it.

It is quite possible, then, that through no fault of his own, our hypothetical chemist might not succeed in obtaining a scholarship. In this case, even though at this period in his career he may have shown an aptitude for research work, his future will depend entirely upon the extent of his private means. If, after taking his degree, he has enough money to remain at the university for another year, there is a possibility of his proceeding further in this line; but if he cannot afford to do this, he will be driven to look out for some position which will enable him to pay his way immediately; and in the latter case he may be lost as far as research work goes, unless he is specially fortunate in the post which he secures.

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A year spent at the university, however, during which he is able to carry out some minor branch of research under the guidance of his chiefs, will bring within his reach one of the most valuable aids which our present system contains, a Research Scholarship or Fellowship.

It is evident to any one who has had to deal with the students of various institutions of the university or technical college type that out of the mass of these men only a few are really gifted with the capacity for research in its higher stages. Some of them are capable of carrying out investigations under direction; but as soon as the guiding hand is withdrawn they collapse, having few ideas of their own. How can we select the best men and give them opportunities of further development? The first idea that occurs to the ordinary mind is to subject the candidate to examination; but a little reflection suffices to show that such a method is worse than futile. An examination, at least if it be conducted on the usual lines, is an excellent method of testing the amount of knowledge which a man has absorbed during his routine training in the pot-hooks of his subject; but as a gauge of his originality it is of no value whatever. In the ordinary examination, the man with a good memory is at a premium; he can recall exactly what he has learned in his textbooks, put it down neatly, and generally display a grip of the printed material and oral instruction which have come his way. But research work is a very different field of mental activity. In it, though a man must be fully acquainted with what has already been done by others in the line of his work, yet all this knowledge is practically worthless to him if he has not got an original cast of mind which will enable him to see his way through some intricate problem without guidance. An examination may suffice to test a man's knowledge of what is already known; but the fitness of a man for research can only be gauged if we know his capacity for converting the unknown into the known.

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Now, it must be frankly admitted that at the present time we have no means of determining *à priori* whether a man will be a successful investigator or not; the only test which we can apply is to allow him to carry out some piece of research and judge him by the results which he obtains. But, as we have already pointed out, it may be necessary to provide the candidate with a living wage during the time in which he is winning his spurs in original investigation.

In connection with our various scientific institutions we have several of these pecuniary aids; but it cannot be said that we have anything like the number which would be required if the whole of our latent research talent is to be fully developed. In addition to the various local scholarships at different institutions, we have three large foundations which devote part of their funds to this work: the Royal Commissioners for the 1851 Exhibition, the Carnegie Trust for the Universities of Scotland, and the Beit Memorial.

The Carnegie Trust is rather more localized in its operations than the others, as its work was limited by its founder to assisting students of Scottish descent and also to aiding the Universities of Scotland in various ways. Its services to research are threefold: one part of its income is devoted to Research Scholarships and Fellowships; another portion is spent in improving the equipment of the four universities; while a third part is used up in assisting investigators in the purchase of apparatus and material for their work.[1] We are here concerned at present with the first of these functions of the Trust.

Suppose that during his undergraduate career a student in one of the Scottish universities shows that he has a leaning towards research. It is probable that he is not sufficiently advanced to conduct independent investigations;

[1] The regulations of the Carnegie Trust are reprinted in the Appendix, as they give a good idea of the manner in which this far-reaching scheme is operated.

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but he may be capable of carrying out some work in conjunction with a Professor or Lecturer. If his qualifications pass muster and, if there are not too many other applications, the Carnegie Trustees may appoint him to a Research Scholarship of the value of £100 per annum. On the other hand, if he has already proved that he is capable of conducting research, he may apply for a Fellowship, which is worth £150 per annum. His application is submitted to experts, who decide whether he is a fit person for appointment, and also whether the line of research which he has chosen will yield results of value. If, after the first year of tenure, he has shown himself well fitted to advance science, the Trustees are prepared to give him a further appointment, which may extend to two years. The Carnegie Trustees have very wisely decided not to throw open a fixed number of Scholarships and Fellowships annually; but they appoint in each year those candidates who seem best fitted for the work. Thus in some years the number of appointments is larger than in others, and no rigid rule is laid down. In this way one of the most serious faults of the ordinary scholarship system is avoided.

The statistics in the table on p. 276 give some idea of the extensive scale upon which the Carnegie Trust operates in the field of chemical research work.

The fact that in the first year of the scheme only two papers resulted is easily explicable; for chemical investigations require a considerable time before the results are reducible to the form of a published paper, and consequently the work of a Scholar or Fellow appointed in 1903 would not in the natural course of events be published until at least a year later. Even with this deduction, however, the table shows that in the eight years 1903–1911 the appointment of forty-five Scholars, twenty-five Fellows and thirty-one Grantees resulted, after taking joint authorship into account, in the publication of more than a hundred and thirty

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	Numbers appointed.			Numbers of papers published by		
	Scholars.	Fellows.	Grantees.	Scholars.	Fellows.	Grantees
1903 . . .	3	3	5	1	—	1
1904 . . .	5	2	3	2	7 (1) [1]	3
1905 . . .	7	3	2	9	4 (1)	1
1906 . . .	7	2	5	9	9	3
1907 . . .	4	5	7	7	8 (1)	9 (7)
1908 . . .	5	3	3	5	9	14 (4)
1909 . . .	9	2	2	2	3	8 (1)
1910 . . .	4	2	2	11	9	7 (4)
1911 . . .	1	3	2	3	7	11 (7)
1903–1911 .	**45**	**25**	**31**	**49**	**56 (3)**	**57 (23)**

original communications embodying the results of chemical investigation. The magnitude of this contribution to knowledge will be recognized when it is pointed out that in 1912 the contributions from the whole British chemical world to the Journal of the Chemical Society amounted to only double this number: two hundred and sixty-six. Thus the Carnegie Trust by its encouragement of research has indirectly in the course of eight years produced a series of results equivalent to half the annual output of the whole kingdom at the present time.

This achievement by itself would be striking; but it must be pointed out that the actual results are even more important. The greater part of this original investigation has been carried out by men who were in the earlier stages of their careers as researchers. Since that time, many of them have continued their work, and though their later publications find no place in the statistics of the Trust, yet the Trustees, by their encouragement of these workers, made the later stages possible; and consequently part of the credit of the more recent publications belongs to the Trust. For example, one investigator, who while an actual

[1] The figures in brackets give the number of papers resulting from the collaboration of two or more workers under the scheme.

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beneficiary of the Trust published fourteen papers, has, since the expiration of his appointment, published about twenty further papers, part of which resulted from a prosecution of the lines of work which he undertook while under the control of the Trust.

Further, it must be pointed out that these results have been obtained in a single science; while the actual field of operations of the Trust is much more extensive in scope, including as it does research in chemistry, physics, biology, medicine, economics, history, and languages.

The funds at the disposal of the Royal Commissioners for the Exhibition of 1851 are drawn from the surplus resulting from that Exhibition. The operations of the Royal Commissioners are very extensive; among other things, they have devoted portions of their funds to a Research Laboratory in the Imperial Institute and an annual contribution in support of this institution; and they have played a very considerable part in the foundation of the Imperial College of Science and Technology at South Kensington.

To Lord Playfair, then one of the Commissioners, is due the scheme of Research Scholarships which has been in operation since 1891. Under this scheme, the Commissioners resolved to devote annually a sum of not less than £5000 towards the establishment of scholarships for the purpose of aiding the development of scientific culture and technical training in the manufacturing districts of the country. It was recognized that very little would be gained unless the scholarships awarded by the Commissioners differed from those already existing; and consequently it was decided that they should be of a higher order and should be awarded for research in the experimental sciences bearing upon the industries. The scope of the scheme thus includes both pure research and technical investigations. Further consideration induced the Commissioners to extend

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their field so as to include within their scheme certain of the Colonial Universities and also the University of London. The network of the Commissioners thus extends over the whole British Empire, and it is probably one of the most comprehensive educational projects ever carried into practice.

Under the regulations laid down by the Commissioners,[1] their scholarships are awarded to students who have passed through a university curriculum and have given distinct evidence of capacity for original research, to enable them to continue the prosecution of science with a view to aiding its advance or its application to the industries of the country. The scholarships are of an annual value of £150, and are ordinarily tenable for two years, the continuation for the second year depending on the reports of the scholars' first year's work being satisfactory to the Scholarships Committee. The Committee may renew a limited number of scholarships for a third year, where they are of opinion that the renewal is likely to result in work of scientific importance. The scholarships are annually awarded to persons primarily recommended by the governing bodies of the institutions to which nominations are from time to time allotted, and the holders are required in the absence of special circumstances to proceed to an institution other than that by which they were nominated.

In 1900, this scheme of scholarships was supplemented by the establishment of a system of Probationary Bursaries, to be awarded in certain cases where the nominee of the institution appears not to be immediately qualified for a scholarship, but gives promise of becoming so after a year's experience of research work. These bursaries are tenable for one year and are of the annual value of £70; and the holder of one may be advanced to a scholarship at the end of his term, if, in the opinion of the Scholarships Committee,

[1] The regulations of the Commissioners are printed in the Appendix.

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his work during his tenure of the bursary is sufficient to entitle him to the award.

In their eighth Report, the Commissioners have given a list of the persons appointed to scholarships since the foundation of the scheme, and appended to each name are notes of the subsequent career of the scholar. With reference to this, the Commissioners state: "This list, containing as it does the names of so many persons who have already distinguished themselves in their professions, furnishes ample evidence of the educational value of the scholarships. Moreover, we believe that for the majority of those whose careers have been attended with so much success, the scholarship came at a time when the special opportunities which it afforded had a marked effect in raising the level in which the scholar could employ his talents in the advancement of science. Indeed, many of the scholars themselves have informed us that, had it not been for the scholarship, they would have been compelled, for want of financial assistance, to discontinue the prosecution of science or take up a position in which their natural ability would have found but little scope. We have further received the strongest evidence from persons interested in the progress of science throughout the empire of the beneficial influence which our scholarships have exerted, not only on the scholars themselves, who have obtained high positions in pure and applied science, but upon the colleges and universities which have benefited by our scheme, inasmuch as the scholarships have tended to raise the standard of the teaching in these institutions."

The list of 1851 Exhibition Science Research Scholars appointed between the years 1891 and 1910 contains three hundred and thirty-six names in all. These appointments cover the fields of chemistry, physics, engineering, botany, geology, astrophysics, entomology, biology, pathology, meteorology, and bacteriology. The chemical appointments

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amount to a hundred and sixty-five, or, in round numbers, fifty per cent. of the total appointments. In examining the subsequent careers of the chemical scholars we must deduct those persons, twenty-two in number, whose scholarships were still current or had recently expired at the time the report was issued. This leaves us with a hundred and forty-three chemical investigators. After the expiry of their scholarships, fifty-eight of these turned to applied chemistry, while the remaining eighty-five took up academic careers of one kind or another.

Let us turn again to the case of our promising student, and assume that he has secured an appointment to a Research Scholarship or Fellowship. He is already a marked man, for he must have attained a very fair standard of excellence to gain such a position. If he comes under the ægis of the Carnegie Trust as a Fellow, he is allowed complete freedom as to where he will spend his period of research; if he be a Carnegie Research Scholar, he must work under the supervision of some senior man approved by the Trustees; while if the student be appointed to one of the 1851 Exhibition Scholarships, he is expected, except under very exceptional circumstances, to betake himself to some new laboratory. The object aimed at in forcing this migration on him is to secure that he becomes acquainted with fresh methods and that he does not become hide-bound in the traditions of his Alma Mater.

He goes to a new institution, then, and spends his time upon original work during the term of his appointment. Here we find a certain divergence in view between the 1851 Commissioners and the Carnegie Trustees. Both bodies make it an essential condition of their appointments that the men selected by them shall devote their time to original work, and shall not, without special permission, hold any other position of emolument conjointly with either scholarship or fellowship; but the Trustees do not

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absolutely bar a Carnegie Fellow from acquiring a certain amount of teaching experience, so long as this does not interfere with the purpose for which he was appointed; the Commissioners, on the other hand, debar their scholars from holding any position beyond the scholarship. Now this is rather a crucial point in the success of a man who intends to take up a position on a University staff at a later period. At the end of his tenure of a Research Scholarship, he is unable to say that he has had any practical acquaintance with teaching; and in consequence he is placed somewhat at a disadvantage as compared with a man who has perhaps done less research work but who has devoted his time to pure teaching. Thus, a scholar, when he applies for a junior post after the expiration of his appointment, runs the risk of being passed over in favour of some less brilliant candidate who has done no original work, but who has devoted himself to reproducing the work of other men for the benefit of junior students. It is only fair to say that this style of appointment is falling into disrepute in most well-directed institutions; but it still remains rather a trap in certain cases.

Let us assume, however, that our hypothetical chemist succeeds in securing an appointment. At this stage in his career he will have spent a matter of ten or twelve years in the study of chemistry, during a large part of which time he will not only have been drawing no salary, but will have been actually out of pocket owing to the expenses of his training. He will probably have acquired a working knowledge of two foreign languages and may be able to speak one fluently. With these attainments, he will be lucky if he secures a post worth £200 a year; if he is less fortunate he may be glad to get anything over £100 per annum. To put the matter bluntly, his market value under the present system is rather less than that of a decent artisan in a shipyard. Further, he now finds that the

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greater part of his time is occupied by teaching, either in the lecture room or laboratory ; and he thus has very little opportunity to continue his research work. On the other hand, if he is given a fairly free hand by his chief, he has a chance of enlisting some of his students to help him in his investigations and of thus aiding them to fit themselves for research work in their turn.

The financial handicap makes itself felt in another direction. Some lines of investigation necessitate the use of considerable quantities of expensive apparatus and materials which are beyond the means of a junior member of a university staff. It is fortunate that we have some means of getting round this difficulty, though they are still much below the level of what is necessary. We have already mentioned the work of the Carnegie Trustees in supplying grants to investigators ; and a similar function is performed by the Royal Society, the Chemical Society, and the British Association for the Advancement of Science. At the same time, the drain upon these funds, which are very meagre, is usually much in excess of their capacity ; so that many deserving cases cannot be considered. Some idea of the effect which such grants have upon the progress of knowledge may be gathered from the following figures published by the Chemical Society in 1907. In the five years 1901–5, the Research Fund Committee of the Society made grants amounting in all to £1163, divided among 151 investigators. As a result of this assistance in the purchase of materials, 203 original papers were published by the grantees. The total number of papers appearing in the Proceedings and Transactions of the Society during the same period was 1830 ; so that the Research Grants probably increased the normal output of research work by roughly ten per cent. From these figures it is evident that these grants are well spent ; and the results obtained by this expenditure suggest that with greater funds at its disposal

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the Society could produce a further increase in the research efficiency of the country. It should be noted that these grants are strictly applied to the purchase of materials and do not in any way benefit or remunerate the grantees personally.

Assuming that our hypothetical chemist has worked hard at both his teaching and his research work, he may aspire to a lectureship, which may fall to him by the time he is about thirty years of age. This will probably bring under his charge a larger number of students who are willing to help him in his work. By this time, he will be fairly well known in the chemical world, not only in this country but also on the Continent. He is now in a position in which important work is expected from him; but at the same time he is more fully occupied with his teaching duties, which have become more responsible. Usually he will find it impossible to carry out both research and teaching within his official hours; so that a good deal of his spare time is swallowed up by his research work. At this period in his career he may be drawing a salary of £200 or £300 a year.

Finally, about ten years later, he may secure a professorship and have control of a department, so that he has a fuller opportunity of carrying out his investigations. He is then in a position to undertake one of those large pieces of work which require for their prosecution the combined efforts of several men; for he will be able to call upon his subordinates, several of whom are probably his own pupils, to assist him in the various parts of the field which he has chosen. But as a counterbalance to this freedom, he finds that the administrative side of his office has grown also; and it is often the case that, instead of having more time to devote to his investigations, his promotion has really diminished his opportunities.

From the foregoing, it will be seen that, on the financial side, a scientific career offers no great inducement to

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beginners; and even the latter stages are not such as would attract a money-grubber. A man with a world-wide reputation for his scientific work may consider himself very lucky if at the age of forty-five he has a salary of £800 a year; and his official income will never increase beyond its original level.

This, however, is not the main stumbling-block in the way of the scientific investigator, who, as a rule, is less eager for money than for opportunities to pursue his original work. The real root of the difficulty lies in the uncertainty of promotion. Under the present system, there are very few posts vacant at any time; and a man may have equipped himself for a higher position long before a vacancy occurs which permits of his advancement. Meanwhile, he may be kept back in his scientific work and not have the full scope for his powers which the command of a large laboratory would give him.

So far in this chapter we have confined our view to the teaching institutions of the country; but it might be suggested that there is another outlet for our investigators to be found in our factories, where their work might meet with a more adequate recognition. A glance at the following facts will show how ill-founded such a hope must be.

In 1902, a committee of the British Association for the Advancement of Science collected information with regard to the number of chemists employed in the factories of this country. The returns made were, of course, not complete; but it appears that a liberal estimate places the total number of chemists thus employed at about fifteen hundred. Now many of these people are not original investigators; for, as Professor Meldola puts it, "Many of our manufacturers dignify with the name of 'chemist' any human testing machine in their employment, and there is no doubt that many men whose daily occupation does not go beyond the valuation of a few staple products or raw materials are

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described as chemists." When we consider that in the German colour industry alone five factories employ between five and six hundred chemists, the greater part of whom are real investigators and not mere testing machines, we can see how very little advantage is being taken of scientific aid by the manufacturers in this country. It cannot be said that such assistance is of little value in technical processes; the origin and progress of the German chemical industry speaks for itself in this connection. The plain truth is that very few of our manufacturing firms have any idea of what the word "chemist" implies. Those which, like Messrs. Burroughs Wellcome & Co., have realized the enormous advantage which a scientific treatment of their problems would bring to them have reaped the success to which their foresight entitled them. At the present day, however, as far as the major part of our industries is concerned there is no inducement for an investigator to enter a factory.

We have now touched upon the main outlines of our present system; and we may devote a few paragraphs to a consideration of the points at which the wastage of the research capacity of the country is greatest nowadays.

Let us begin with the chemist in his student days. In most of our universities, owing to lack of sufficient staff and also on account of limited laboratory space, we find that the students of all types are dealt with in bulk during their first year of chemistry. Now, this implies that medical students and engineers pass through the same kind of course as that which is offered to a student who is going to make chemistry the study of his lifetime. Naturally some attempt is made, where possible, to segregate these three sections as far as possible, and train them in the particular branches of chemistry which are going to be of most importance to them later in life; thus engineers are given a fuller training in metallurgy than their fellow-students; while medicals, instead of this, take up the study of organic

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chemistry. This segregation, however, cannot be more than a makeshift under our present system; what is really required is that each of these three classes of student should be provided with a complete and connected course devised entirely with a view to his future work, so that the time at his disposal could be completely and properly utilized. The system is especially feeble in its attempt to cater for the pure science student; for, at the beginning of his career, he is in most institutions grouped along with the others, with the result that his first year's work is spent in acquiring the elements of medical and engineering sections of chemistry in addition to the principles of the subject; instead of which he ought to be handed over to independent teachers who would give him a training more suitable to his future work.

The student suffers also in his first year from having to acquire a mass of facts for examination purposes; and owing to this work having to be done at high pressure on account of the length of the medical curriculum, the science man in the same classes has to cram with the rest. This is certainly by no means a happy method of instilling into him a liking for his subject. Though in the larger technical colleges it might be possible to improve this system to some extent, owing to their not explicitly having to cater for the first-year medical student; still, they are handicapped like the universities by lack of sufficient teaching staff and fuller accommodation for laboratory instruction, which would be necessary if the proper division of the students into their respective classes were to be carried out.

Then again, owing to the narrow financial basis upon which most of the teaching institutions stand, it is necessary to attract as many students to them as possible; and the natural result of this is that the governing bodies, either from force of habit or from pecuniary necessity, tend more and more to regard their institution as a business concern which must get the fees in at any cost.

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In his Presidential Address to the Chemical Society in 1907, Professor Meldola, F.R.S., drew attention to the results of such methods. "In the first place, there is introduced that most baneful system of teaching the subjects in 'classes' so that a syllabus qualifying for some particular examination may be gone through in a certain time. It is quite unnecessary to point out here that individual originality or the spirit of research can never exist in such an atmosphere. And the most serious aspect of the case is that, not only is a subject taught in this way never really assimilated so as to become a living principle with the student, but, what is far worse, the teacher himself, however original and zealous at the outset, is bound, after running in this groove, sooner or later to undergo deterioration. . . . The prevalence of the 'business' spirit among the committees and governing bodies gives an exaggerated importance to what may be called the office staff—the registrars and clerks. The work of the office staff is capable of being appreciated by the average committeeman, while the work of the scientific staff is generally beyond his comprehension, excepting so far as it can be measured by financial gain to the institution. . . . Now I am not decrying the business faculty as such; it is an essential qualification for the proper government of any educational establishment. . . . But that is no reason why the teaching staff should be, as is frequently the case, subordinated to the office staff. It is not sufficiently realized that men of business and administrative ability are by no means rarities, while really good teachers of science are much scarcer, and men who combine both the qualifications of a good teacher with the inspiring zeal of the investigator are rarest of all. Now if, as was professedly the case, the modern departure in technical education had for its object the improvement of the industries, then it is sufficiently well known to us here that the future of our subject is with the men of the latter class, and the joint

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exertions of all the registrars and clerks, backed by the efforts of the most skilful chemical pedagogues who get through their syllabus within the session and earn the largest grants or score the highest percentage of successful 'passes,' will never raise the level of this country either in chemical science or chemical industry. . . . I will not venture to intrude my opinions concerning the vitalizing influence of research upon other scientific subjects, but with regard to our own I have not the least hesitation in declaring the belief that a school of chemistry, which is not also a centre of research, is bound to degenerate and to become a mere cramming establishment not worth the cost of maintenance. It is easy enough to follow the actual course of the degeneration process in such an institution. The teacher, who may be a man of real ability and who has entered with the hope of finding time and opportunities for research, finds himself, sooner or later, in the position of a chemical schoolmaster. The predominance of the business influence in the institution not only leads, as I have already indicated, to the lowering of the level of instruction and to his own consequent degeneration, but he is, as a further consequence, so over-weighted with business and administrative work that these, superadded to his teaching duties, leave him neither time nor energy for original work. The spirit of research within him is strangled by officialism, and his teaching faculties are deadened by the monotonous toil of the annually recurring drudgery of routine teaching. He has not even time to educate himself by keeping in touch with the progress of his subject . . . his research faculty is lost to the country, and his teaching becomes less and less efficient as he falls more and more behind the actual state of knowledge—he undergoes submergence. . . . In the present state of affairs, one can only marvel at the fact that so many men of ability can be found willing to take service in these newer institutions [the Technical Colleges],

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the more especially as, apart from the absurdly inadequate remuneration often given to the chiefs of the chemical departments, the payment of the subordinate members of the staff is generally on a scale which is nothing short of a scandal to the wealthiest of European nations. Considering the long course of training necessary to produce a competent teacher or demonstrator, and in view of the actual amount of work expected from these men who, by virtue of their attainments and position, are compelled to live up to a standard of high respectability, it seems almost incredible that the average scale of remuneration should not exceed the wages earned by an artisan, and is often below that standard. . . . The practical result from our point of view is again the crippling of the research faculty; the chiefs are inadequately supported, and the subordinates have to work overtime as examiners or in some other capacity in order to make a 'living wage.' It is needless to say that under such conditions there is wholesale destruction of research talent going on to the ultimate detriment of our country."

It is unnecessary to labour these points further, but we may take a crucial instance which will show the weakness of the present system. It will hardly be credited by those unacquainted with our present machinery; but, nevertheless, it is an actual fact that we utilize our greatest investigators to a large extent in delivering lectures on the elements of chemistry to first-year students. There is, no doubt, something to be said for bringing the junior student into direct contact with the masters of science; but the idea is really pushed too far when we expect a scientific genius, whose time is valuable in the extreme, to devote, say, twenty per cent. of his working day to the instruction of beginners. It would be much better if some one of intermediate standing took over the main portion of this work and the eminent investigator lectured at intervals to the juniors on detached portions of the subject, instead of grinding through a

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systematic course day in and day out throughout the session. In this way the juniors would be quite as efficiently instructed; they would at intervals come into contact with the leaders of their science; and the latter would be able to devote themselves more freely to the work which they alone can do.

The matter resolves itself into a very simple alternative. If we have a great investigator, do we propose to give him full facilities for the work which probably he alone can carry forward; or do we propose to employ him as a kind of hybrid between a chemical schoolmaster for beginners and an administrative official? One of the greatest of present-day investigators—who is also one of the finest lecturers and teachers—has stated that his duty is to "make chemistry and not chemists." This may be an extreme statement of the case; but it indicates clearly the two paths. It must not be forgotten that any great researcher, apart altogether from his actual work in lecturing, exercises in his department an influence which can scarcely be estimated. A laboratory in which chemical history is being made day by day has an intellectual atmosphere which stimulates the student and aids him in his work to an extent which no mere teaching can ever effect; discussion is evoked and problems are thought out by the student which would hardly attract his attention if they came to him as the mere dry bones of a text-book; and such discussions are of infinitely greater service to science than any reading of text-books can possibly render.

It needs no great perspicacity to deduce that the root of the whole matter is to be found, as in most things, in finance. Clearly we have not sufficient funds at our disposal to utilize even a reasonable percentage of the human material for research which this country is capable of producing. When it is realized that under our present system we have no means of utilizing even the foremost of

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investigators on pure research work; but are forced to hamper them with teaching appointments because there is no other way of fitting them into the system, the absurdity of our present machinery becomes manifest. It is very much like saying that no one should become a director of the Bank of England unless he is prepared to carry in coals for the office fire each morning.

Now, money may be forthcoming from two sources. We have already seen that owing to the generosity of Dr. Andrew Carnegie and to the Beit bequest a very considerable improvement in the state of affairs has been accomplished; and it seems not impossible that further efforts may be made to better the system by private persons. This is not the place to discuss such suggestions in detail; but lest it be said that no concrete scheme has been submitted, the rough sketch of one is given in an appendix.

There remains the possibility of Government aid. At the present time we pour out money like water on elementary education, but dribble it out reluctantly when we come to deal with the higher levels. In 1911, the Parliamentary Grant for elementary education in England and Wales amounted to over eleven millions sterling; while the corresponding Grant for University purposes was less than two per cent. of this total. Considering the relative values to the industrial resources of Britain of a labouring man educated up to the primary school standard on the one hand and on the other an investigator such as the late Sir W. H. Perkin whose work led to the foundation of the aniline dye industry, it seems probable that a slight increase in the Grant towards higher education would repay the country better than a further rise in the assistance now given to primary education.

What is urgently needed is a ladder by means of which the most deserving investigators, no matter from what class they come, can reach the highest appointments with certainty,

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provided they can prove their worth. So long as promotion is out of the reach of the obviously unfit, the nation as a whole would gain enormously by the institution of such a system. It is not so much a question of increasing the pay of the higher positions, but rather an increase in their number and in that of the intermediate posts which is required. If we can expand the scientific output of the nation, we shall either directly or indirectly further its commercial prosperity. The words of Huxley, spoken a generation ago, are equally true to-day.

"I would make accessible the highest and most complete training the country could afford. Whatever that might cost, depend upon it the investment would be a good one. I weigh my words when I say that if the nation could purchase a potential Watt, or Davy, or Faraday, at a cost of a hundred thousand pounds down, he would be dirt-cheap at the money. It is a mere commonplace and everyday piece of knowledge that what these three men did has produced untold millions of wealth, in the narrowest economical sense of the word."

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APPENDIX A

ON THE BETTER UTILIZATION OF THE RESEARCH CAPACITY OF THE COUNTRY

OUTLINES OF A SCHEME FOR THE IMPROVEMENT OF RESEARCH CONDITIONS.*

The Objects of the Scheme.

(1) Any complete scheme for the better utilization of the research talent of the country must provide for the following operations:—

(*a*) The selection of students capable of research work.

(*b*) The assisting of graduates who have proved their value by original investigations either independently or under the direction of others.

(*c*) The provision of suitable posts in later years for these graduates so that their researches may not be interrupted.

(*d*) The establishment of research professorships.

(*e*) The subsidizing of investigators by means of grants to be applied exclusively to the purchase of expensive apparatus and materials.

(*f*) The equipment and maintenance of research institutes.

(*g*) The dissemination of information and criticism in the form of monographs, etc.

Note.—With regard to headings (*a*), (*b*), (*e*), (*f*) and (*g*), the only body which covers the field at the present time is the Carnegie Trust for the Universities of Scotland. Its Regulations furnish a model for any future effort in this direction; they will be found reprinted in Appendix B. With regard to the heading (*f*), the Carnegie Trustees, in addition to assisting to maintain a Research Institute in Edinburgh, devote a portion of their funds to the equipment of University laboratories, and to meeting special research expenses of the University staffs; and in this way they have done much to improve the position of research in

* This Scheme is intended to be taken in conjunction with Chapter XV.

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Scotland. The Royal Commissioners for the Exhibition of 1851, as well as the Beit Memorial, expend part of their funds under the heading (*b*); while the Royal Society, the Chemical Society, and the British Association for the Advancement of Science furnish sums under the heading (*e*). Under the heading (*c*) the case of technical research must be included.

Undergraduates.

(2) The undergraduate represents the most junior grade of the research personnel. A student in his final year, if he has any capacity for original investigation, has usually attained to a standard of knowledge and aptitude in practical work sufficient to allow him to conduct research under the direction of one of his seniors. At this stage in his career, it would not be necessary to provide him with a scholarship except under very special circumstances. If a beginner does not care to carry out investigations unless he is paid, the chances are that he is not really interested in the work. It might be possible to reward good work by means of prizes; but it would be preferable to hold out the prospect of an appointment to a Scholarship or Fellowship after graduation.

Graduates.

(3) In the case of graduates two possible cases present themselves. During his undergraduate career a man may feel that it is essential for him to pass his examinations without risk of failure; he may thus be unable to devote any time to research; consequently he may be free to undertake investigations only after he has secured his degree. At the present time many such men are lost on account of financial reasons; they are unable to spend another year in the laboratory even if they wished to do so. Since this type of graduate is only a beginner at research work, it would not be necessary to appoint him to a highly paid post. He should be given enough to keep him from actual financial strain, and his appointment should be a short one, so that if he prove a failure he may be rejected with as little loss of time as possible. A six-monthly appointment to a Scholarship worth £40 and renewable for another half-year would be ample. During the tenure of such a Scholarship he would be expected to work under the supervision of a senior man, and to devote the whole of his time to research work.

(4) The second group of graduates contains men of a more advanced type, being drawn from the ranks of undergraduates

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who have already some research work to their credit. Such men, if they have acquitted themselves creditably, should be appointed to more valuable Scholarships or Fellowships, and the standards of attainment and tenure should be similar to those set up by the Carnegie Trustees, as these appear to be perfectly satisfactory. There should be Senior Scholarships of the value of £120 per annum and tenable for one year; if the report on the Scholar's work were satisfactory he might, in the ensuing year, be appointed to a Fellowship of £150 per annum, with the chance, in the third year, of an appointment to a Senior Fellowship at £180 per annum. One of these years he should, unless under exceptional circumstances, be expected to spend in a laboratory other than that from which he graduated; and he should be required to submit a report upon this laboratory and its work as part of his position. Senior Scholars and Fellows appointed under this section of the scheme would be expected, except under exceptional circumstances, to conduct independent researches, and not to work under the direction of others. Part of the duties entailed on them would be the delivery of very short courses of lectures on the lines of research upon which they were engaged, so that advanced students might profit from their knowledge and practical experience.

Applied Research.

(5) Among the beneficiaries of the scheme it is probable that a number of investigators would be found whose talents lay in the field of the application of their investigations to commercial processes. In their case, successful investigation would probably result in their being able to acquire patents for any improvements which they succeeded in introducing into the industrial field; and the greater their success, the less need would they have for support from the research funds. This state of affairs might be dealt with by insisting that a full declaration of royalties, etc., derived from the working or sale of patents, should be submitted to the authorities; and the research fund would contribute to its beneficiary only such sums as would bring his income up to the level of that paid out of the funds to workers of the same standing who were engaged in pure research. In this way applied science would be placed on the same footing as pure science; while at the same time an incentive to further work would be given if it were understood that the applied science appointments were terminated definitely in a fixed number of

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years, so that no beneficiary could remain indefinitely in a position to draw support from the funds. In this way it would be possible to encourage men who had been trained in the junior positions to devote themselves entirely to applied work, and thus there would be a continual shedding of beneficiaries at a certain point in their careers, so that it would not be necessary to provide a higher position for every junior who entered into the scheme. Conditions could be drawn up which would allow manufacturers to employ beneficiaries of the scheme upon certain problems in connection with their factories, and in return for this the manufacturers would be required to pay to the research fund sums depending upon the work and capacity of the beneficiary employed by them.

Readers.

(6) After a man has pursued his research work with such success as to secure an appointment to a Senior Fellowship, he would be an outstanding investigator, and should be given an opportunity of extending his influence. Under the present system this is a matter which depends too much upon chance appointments. This difficulty could be avoided by creating a post of Reader, tenable only by those who have attained to the grade of Senior Fellow. The salary might be £250 per annum in the first year, rising, in case of reappointment, in succeeding years by increments of £25. The duties of the position would be twofold. On the research side, in addition to his conducting his own researches, the Reader would be placed in charge of a group of scholars and undergraduate workers, while the main bulk of the advanced teaching of chemistry to Pure Science students would also be allotted to one or more Readers. The object of this teaching appointment is to bring the investigator into touch with the undergraduates, in order that he may be able to decide which of them is capable of original research. The Reader would be held responsible for the progress of the investigators under his charge, and would be expected to weed out the less capable ones after a fair trial. In this way he would be trained in responsibility and the conduct of the researches of his juniors, while he would also increase his experience of teaching.

Research Professors.

(7) The chief posts under the scheme would be filled by Research Professors, whose duties would include: (1) the

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prosecution of original research; (2) the directing of research institutes and their personnel; (3) the supervision of the work of the junior grades of beneficiaries; (4) the delivery of courses of lectures, not necessarily exceeding 25 per session, upon the subjects with which they were actually in touch. Such men could be drawn from the ranks of the Readers, or could be chosen from among exceptionally able investigators outside the scheme. It would be undesirable to appoint them for life in the first instance, but security of tenure would be understood if their work did not fall off. The case might be met by appointing them in the first instance for four years at a salary of £400 per annum; then if they fulfilled the expectations originally formed, they might be reappointed for a further four years at £500. Another term of four years might follow at £700; after which they might be giving a life appointment at this salary. In this way they would, if they had been regularly reappointed, have produced twelve years of first-class work, and such a record would guarantee that they would be equally efficient and conscientious during the remainder of their careers.

Grants for Research Materials.

(8) Grants of funds in aid of research could be made to those beneficiaries or others who could show good cause for their demands. Any permanent apparatus purchased with the funds would become the property of the authorities of the scheme, and would be lent to the worker under proper guarantees for its return in good condition. No portion of the grants should be expended either in payment of assistants or in personal expenses of any kind. Where an investigator required assistance, it might be possible to allot to him a beneficiary of some junior grade who would work under his directions, subject to the control of the authorities.

Research Institutes.

(9) Where it was found desirable, Research Institutes could be endowed; but in such cases care would need to be taken that there was actually a field for these institutions, as the segregation of research work from the main line of students would not have a beneficial effect from the point of view of the scheme as a whole. In such Institutes qualified workers could be admitted and provided with materials at reasonable fees; and in them facilities might be provided for those investigators who were working out practical problems for manufacturers, as in such

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Institutes it would be possible to ensure that secrecy was observed.

The Publication of Monographs, etc.

(10) Until quite recently British scientific literature suffered by comparison with German publications, owing to the fact that while a large non-German public buys monographs in German, there is not the same foreign support for English publications. In this way it is difficult to secure the publication of scientific works in English. The Carnegie Trust has, in certain cases, stepped into the breach and financed the printing of important contributions to knowledge, and it seems probable that an extension of such methods might lead to even better results in future. At the present time, many scientific works in English are published at high prices, and consequently very few people can afford to buy them, whereas if they were brought out more cheaply there might be a larger sale, and consequently a wider dissemination of knowledge. It might be possible to come to some arrangement with a firm of publishers whereby a reasonable profit would be guaranteed to the publishers out of the funds of the scheme if the works in question were published at a lower price than at present; if under these conditions the books had a sale sufficient to make them profitable, the funds of the scheme would not be called upon to any extent. Under such conditions it might be possible to issue absolutely up-to-date works in very small and frequently revised editions, which at the present time is almost an impossibility.

Control.

(11) This could best be carried out on the lines at present adopted by the Royal Commissioners for the Exhibition of 1851 and the Carnegie Trust for the Universities of Scotland. A moderately large board of men, distinguished partly in the scientific field and partly in other spheres of activity, would elect an Executive Committee which would carry out the actual work. Adjoined to this would be an Advisory Board of experts, a fixed number of whom might retire each year and be replaced by others. An Executive Secretary who would have power to deal with urgent cases upon his own authority, but whose decisions would be subject to ratification by the other authorities in due course, would be an essential factor in the scheme, as many cases must arise which cannot be postponed until a meeting can be called. Finally, the machinery would need to be very elastic, as any attempt to work on red-tape lines would be foredoomed to failure.

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APPENDIX B

REGULATIONS FOR SCHOLARS, FELLOWS, AND GRANTEES OF THE CARNEGIE TRUST FOR THE UNIVERSITIES OF SCOTLAND

I. Scholarships in Science and Medicine.

The Executive Committee are prepared to consider applications for Carnegie Scholarships in Science and Medicine on the following conditions:—

I. A Scholar must be a graduate of a Scottish University who desires to devote himself to higher study and research in some department of science or medicine.

II. A Scholarship shall be of the annual value of £100, payable by quarterly instalments in advance, the second and subsequent instalments being payable on the receipt of a satisfactory report by the Scholar and a certificate from the authority under whose supervision the Scholar has been working. The Executive Committee reserve power to suspend or withhold payment, if not satisfied with the progress or conduct of the Scholar.

A Scholarship shall not be held along with any other scholarship or position of emolument, except with the sanction of the Executive Committee.

In the event of a candidate holding or obtaining any other scholarship or position of emolument, the Executive Committee may, in place of granting or continuing to grant a Carnegie Scholarship, supplement the amount of such scholarship or emolument so as to bring the income up to at least £100, the candidate thereby ranking, and coming under the same regulations, as a Carnegie Scholar.

III. A Scholar shall ordinarily be expected to devote his whole time to the purpose for which the Scholarship is awarded. If he proposes to undertake other work during his tenure of the Scholarship, he must define such work precisely, and obtain the sanction of the Executive Committee.

IV. A Scholarship shall ordinarily be tenable for one year;

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but it may be renewed for a second year, if the Executive Committee deem this expedient.

V. A candidate must be nominated by a Professor or Lecturer in a Scottish University, or by a Teacher in Scotland recognized for the purpose of graduation by a Scottish University, under whose supervision, unless other supervision be approved by the Executive Committee, he shall work during his tenure of the Scholarship.

The Nomination Form, to be signed by the nominator and the candidate, must contain information on the following points, all of which will be taken into consideration in estimating the relative claims of candidates :—

(1) The age of the candidate, his career as a student, and his knowledge of modern languages.

(2) His special fitness for the work proposed.

(3) His programme of study and research during his tenure of the Scholarship, and where and under whose supervision he proposes to work.

(4) Whether the candidate proposes to undertake other work during his tenure of the Scholarship; and, if so, the character of the work proposed, and the demand on his time which it will involve.

(5) Whether the candidate holds, or expects to hold, any other scholarship or position of emolument; and if so, particulars of the amount and duration of such scholarship or emolument.

(6) The names of two or more authorities, other than the nominator, to whom the Executive Committee may refer as to the qualifications of the candidate.

The nominator must, if required, satisfy the Executive Committee that the candidate, if appointed to a Scholarship, will be provided with the facilities and supervision necessary for carrying out his programme of study and research, and that such information will be furnished on his progress and conduct as may seem expedient to the Executive Committee.

VI. By accepting a Scholarship a Scholar comes under an obligation to submit such reports on the progress of his work as the Executive Committee may require.

II. Fellowships in Science and Medicine.

The Executive Committee are prepared to consider applications for Carnegie Fellowships in Science and Medicine on the following conditions :—

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I. A Fellow must be a graduate of a Scottish University who has given evidence, preferably by work already published, of capability to advance science or medicine by original research, and who desires to devote himself further to this work.

II. A Fellowship shall be of the annual value of £150, exclusive of such special expenses in connection with his research as the Executive Committee may allow. Payment shall be made by half-yearly instalments in advance; but the Executive Committee reserve power to suspend or withhold payment, if not satisfied with the progress or conduct of the Fellow.

A Fellowship shall not be held along with any other fellowship or position of emolument, except with the sanction of the Executive Committee.

In the event of a candidate holding or obtaining any other fellowship or position of emolument, the Executive Committee may, in place of granting or continuing to grant a Carnegie Fellowship, supplement the amount of such fellowship or emolument so as to bring the income up to at least £150, the candidate thereby ranking, and coming under the same regulations, as a Carnegie Fellow.

III. A Fellow shall ordinarily be expected to devote his whole time to the purpose for which the Fellowship is awarded. If he proposes to undertake other work during his tenure of the Fellowship, he must define such work precisely, and obtain the sanction of the Executive Committee.

IV. A Fellowship shall ordinarily be tenable for two years, the extension to a second year being dependent on the receipt of a satisfactory report by the Fellow on his work during the first year; but it may be renewed for a third year, if the Executive Committee deem this expedient.

V. A candidate must furnish the Executive Committee with information on the following points, all of which will be taken into consideration in estimating the relative claims of candidates :—

(1) His age, his career as a student, and his knowledge of modern languages.

(2) His special fitness to conduct the research proposed, and his previous experience, along with any published accounts of work already done.

(3) His programme of research during his tenure of the Fellowship; where he proposes to work; and whether he proposes to work independently or in collaboration.

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(4) Whether he proposes to undertake other work during his tenure of the Fellowship; and, if so, the character of the work proposed, and the demand on his time which it will involve.

(5) Whether he holds, or expects to hold, any other fellowship or position of emolument; and if so, particulars of the amount and duration of such fellowship or emolument.

(6) The names of two or more authorities to whom the Executive Committee may refer as to his qualifications.

(7) An estimate, as detailed as is possible, of special expenses, if any, required in connection with the research he proposes to undertake.

VI. By accepting a Fellowship a Fellow comes under an obligation to submit such reports on the progress of his work as the Executive Committee may require.

The publication, in some form, of an account of the results of his research will be expected in all cases.

III. Grants in Aid of Research.

The Executive Committee are prepared to consider applications for Grants in aid of Research on the following conditions:—

I. An applicant for a Research Grant must be a Professor, Lecturer, or Assistant in a Scottish University, a Teacher in Scotland recognized for the purpose of graduation by a Scottish University, or a Scottish University Graduate resident in Scotland.

II. A Research Grant is not intended to take the place of such provision as the University Courts may make out of the Grants for Permanent Equipment under the scheme of "Grants for five years to the four Universities of Scotland."

III. An applicant must furnish the Executive Committee with information on the following points, all of which will be taken into consideration in estimating the relative claims of applicants:—

(1) His experience in research, with copies of or references to any published papers, or, if he has no papers to offer, with references to two or more authorities who are acquainted with his qualifications.

(2) The nature of the research in which he desires to engage, and the results expected to follow therefrom.

(3) The period over which the proposed research is likely to

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extend, and the approximate amount of time which he expects to be able to devote to it.

(4) A statement of special requirements for the proposed research, with a detailed estimate of the cost.

(5) Whether he has received, or is receiving, any grant from any other source for the same object; and if so, what results have already ensued from his investigations.

IV. By accepting a Grant an applicant comes under an obligation to send to the Executive Committee, when required, a report containing—(*a*) a brief statement (not necessarily for publication) showing the results arrived at, or the stage which the inquiry has reached; (*b*) a statement of the expenditure incurred; and (*c*) copies of or references to any papers in which results of the research have been printed.

The publication, in some form, of an account of the results of the research will be expected in all cases. In the publication of any work for which financial assistance has been received from the Trust, due acknowledgment of such assistance shall be made, and Scholars and Fellows under this Scheme shall use the title "Carnegie Research Scholar" and "Carnegie Research Fellow" respectively.

V. Instruments of permanent value purchased by means of the grant shall, at the conclusion of the research or at such other time as the Executive Committee may determine, be placed under the care and at the disposal of the institution in which the research has been conducted, provided that the Executive Committee may, if they see fit, request their return.

All apparatus of permanent value provided by the Trust for purposes of research shall be labelled and catalogued as belonging to the Trust, and an account of such apparatus shall be sent annually to the Executive Committee.

VI. An application for a Grant to two or more collaborators in the same research must be signed by each; but they shall appoint one of their number who shall be responsible for furnishing the report, for receiving and disbursing the money, and in general for the conduct of the research.

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APPENDIX C

ROYAL COMMISSION FOR THE INTERNATIONAL EXHIBITION OF 1851

SCIENCE RESEARCH SCHOLARSHIPS.

General Regulations.

(1) The Scholarships are intended, not to facilitate attendance on ordinary collegiate studies, but to enable Students who have passed through a College curriculum and have given distinct evidence of capacity for original research, to continue the prosecution of Science with the view of aiding its advance, or its application to the industries of the country.

(2) The Scholarships are of £150 a year, and are ordinarily tenable for two years, the continuation for the second year being dependent on the work done in the first year being satisfactory to the Scholarships Committee.

(3) A limited number of the Scholarships are renewed for a third year where it appears that the renewal is likely to result in work of scientific importance.

(4) Candidates are recommended by the governing bodies of the Universities and Colleges to which Scholarships are allotted, and the recommendations are considered and decided upon by the Scholarships Committee.

(5) The Candidate must be a British subject.

(6) The Candidate must have been a *bonâ fide* student of Science in a University or College in which special attention is given to scientific study for a term of three years.

(7) The Candidate shall be eligible for a Scholarship provided (1) that he has spent the last full academic year immediately prior to the time of nomination as a student in any faculty or scientific department of that Institution by which he is nominated, or (2) that he has been a student of such Institution for a full academic

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year ending within twelve months prior to the time of nomination, and since ceasing to be such student has been engaged solely in scientific study.

The word "student" in the preceding regulation must be understood as comprehending one engaged in undergraduate or post-graduate work.

(8) The Candidate must indicate high promise of capacity for advancing Science or its applications by original research. Evidence of this capacity is strictly required, this being the main qualification for a Scholarship. The most suitable evidence is a satisfactory account of a research already performed, and the Commissioners will decline to confirm the nomination of a Candidate unless such an account is furnished, or there is other equally distinct evidence that he possesses the required qualification.

(9) A Candidate whose age exceeds 30, will only be accepted under very special circumstances.

(10) A Scholarship may be held at any University in England or abroad, or in some other Institution to be approved by the Commissioners. Every Scholar is, in the absence of special circumstances, required to proceed to an Institution other than that by which he is nominated.

(11) The principal work of a Scholar must be a research in some branch of Science, the extension of which is important to the national industries.

(12) Scholars are required to devote themselves wholly to the objects of the Scholarships, and are forbidden to hold any position of emolument.

(13) Scholars are required to furnish reports of their work at the end of each year of the tenure of their Scholarships. At the expiration of each Scholarship the reports of the Scholar are referred to an eminent authority on the subject treated of, who furnishes an opinion thereon to the Commissioners.

(14) The Scholarship stipend is payable half-yearly in advance, but £25 is reserved from the fourth payment until the Scholar has made a satisfactory final report.

Regulations for Probationary Bursaries.

(1) A Bursary is intended for the maintenance for one year of a Student who proposes to become a Science Research Scholar under the scheme of the Commissioners at the expiration of the period covered by the Bursary, in order to afford him an

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opportunity of proving his power of carrying on independent research. The authorities of an Institution recommending a Student for a Bursary will be assumed to have satisfied themselves that he *bonâ fide* intends to accept a Scholarship if subsequently appointed to one.

(2) An applicant for a Bursary must, except as to evidence of capacity for original research, fulfil all the conditions for the time being laid down for appointment to a Science Research Scholarship. He must have passed a B.Sc. examination (or its equivalent) with Honours before the commencement of the period covered by the Bursary. His age must not exceed 25, except under very special circumstances.

(3) A Bursary is tenable for one year, and is of the value of £70, payable by half-yearly instalments in advance, the second instalment being payable on receipt of a certificate from the Professor, under whom the holder has been working, that he has faithfully performed his duties

(4) A Bursary will be awarded on condition that the nominating Institution undertakes to provide for the holder facilities for conducting research, and the requisite supervision, free from charge and incidental expenses.

(5) The holder of a Bursary shall devote himself exclusively to research and work preparatory to research, and none of his time shall be spent in assisting a teacher in his duties. The holder of a Bursary must not hold any other Bursary, Scholarship, or position of emolument.

(6) The holder of a Bursary shall on or before 1st May in the year of tenure send to the office of the Commissioners an account of the research work performed by him, together with an application for appointment to a Science Research Scholarship. The Commissioners will expect to receive from the Professor under whom the holder of the Bursary shall have worked, a confidential opinion as to his capacity and qualifications.

(7) The Commissioners may either appoint the holder of a Bursary to a Science Research Scholarship, or at their absolute discretion decline to appoint him and, in the latter case, shall not be called upon to state any ground for their decision.

(8) A Science Research Scholarship, if granted, shall be held on the usual conditions attached to the Scholarships, or on any special conditions which the Commissioners may impose. But a Scholar who previously to his appointment has held a Bursary shall not be eligible for exceptional renewal of his Scholarship for a third year.

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INDEX

A

ADSORPTION, possibility of immunity phenomena being akin to, 73
Agglutination of bacteria, 65
Agricultural chemistry, 19
Agriculture, bearing of colloid chemistry on, 96
Air, liquefaction of, 150
Alkali industry, 256 ff.
Allotropy, 4
Alpha-rays, 175, 176
Amalgamation process for gold-extraction, 45
Amboceptors, 70
Ammonia-soda process, 260
Ammonium cyanate, Wohler's researches on, 2
Analysis, chemical, 253
Anaphylaxis, 74
Anhydrite, 24
Aniline mauve, 11
Anti-ferment, 53
Anti-toxin, 54 ff., 58, 64
Anti-toxin, diagram showing action of, 64
Applied chemistry, 5, 9, 11, 37 ff.
—— research, endowment of, 295
—— research, factors in, 261
Arc, electric, fixation of nitrogen with, 240 ff.
Arc spectra, characteristics of, 104–5
Argon, discovery of, 106–7
Arrhenius, 73
Arsenic, drugs derived from, 79
Astrakanite, 27
Astronomy and chemistry, 22
Astrophysics and the evolution of elements, 202
Asymmetric agent, light as a, 133
—— carbon atom, 126
—— substances, genesis of, in Nature, 132
Asymmetry, 125, 130, 133
——, its influence upon sensation, 130
—— and vital action, 133
Atom, asymmetric carbon, 126
Atomic weight, 138

B

Bacilli, destruction of, by phagocytes 73
Bacteria, clumping of, 65
——, killing of, by serum, 67
——, their action on the body, 69
——, toxins produced by, 56
Battery, electric, 18
Becquerel, 166
—— rays, 167 ff.
—— —— and X-rays, 177–8
—— ——, nature of, 171 ff.
—— ——, physiological effects of, 178
Beet-sugar, 48 ff.
—— industry, statistics of, 50
Beit Memorial, 274
Beta rays, 175, 176
Biochemistry, 20
Biological applications of blood reactions, 76
Biology, chemical tests in, 77
Biot, 36, 122
Birkeland-Eyde process, 240 ff.
Bleaching powder, 259
Blood, constituents of, 51–2
—— corpuscles, 51
——, functions of, in animal body, 51
——, methods of distinguishing one animal's from another's, 76
Blowpipe, oxy-acetylene, 39
Blue mould, 128–9
Bolometer, 100
Bordet, 71
Boron carbide, 39
Bredig, 90
British Association for the Advancement of Science, chemical statistics, 284
Bröggerite, 156
Bromine and its congeners, 140 ff.

Digitized by Google

Brown, 93
Brownian movement, 92 ff.
—— ——, method of studying, 94
Buchner's experiments on enzymes, 31
By-products, importance of, in manufacturing processes, 5

C

Calcium carbide, 88, 231
—— fluoride under cathode ray bombardments, 207 ff.
Cameron, 203, 206
Cane-sugar, 48
Carbide, calcium, 231
Carbides, 38–9
Carbohydrates, 243
Carbon arc, spectrum of, 101, and Plate I
—— atom, asymmetric, 126
—— compounds, chemistry of, 5
—— compounds, classification of, 7
—— dioxide, 39
——, incandescent solid, spectrum of, 102
—— monoxide, 40
—— vapour, spectrum of, 102
Carborundum, 39
Carnallite, 24
Carnegie, Dr. Andrew, 291
—— Trust for the Universities of Scotland, 274 ff.
—— Trust's Regulations, 299
Catalysis, 30 ff.
Cathode ray bombardment, 207
Cavendish, 143–4
Celluloid, non-inflammable, 47
Ceria, 40
Cerium, 43
Cerium-iron alloy used in tinder-boxes, 44
Chalcolite, 168
Charcoal, 4
Chemical constitution, 255
—— research, organization of, 263 ff.
—— ——, methods of, 248 ff.
—— Society, Annual Report on Chemistry, 7
—— —— Research Fund, 282
—— statics and dynamics, 13 ff.
—— structure, 119
Chemiotaxis, 74
Chemistry and astronomy, 22
—— —— geology, 23
—— —— industry, 37 ff.
—— —— pathology, 33
—— —— physiology, 28
—— —— vital action, 245–6
Chemistry, agricultural, 19
——, applied, 5, 9, 11, 30, 87 ff.
——, division into organic and inorganic, 2
——, extension of its field into other sciences, 21
—— in space, 18, 118 ff.
——, internal ramification of, 1 ff.
——, mineralogical, 19
——, physical, 13
——, three-dimensional, 18
Chemotherapy, 77
Chili mineral beds, statistics of, 238
—— saltpetre, 237–8
Chlorine and its congeners. 140 ff.
Classification of carbon compounds, 7
—— —— elements, 3
Cleveite, 156
Clumping of bacteria, 65
Coagulation, 84
Coal, 227 ff.
Coal-tar dyes, history of their discovery, 11
—— industry, 10 ff.
Collie, 157, 192, 207 ff., 212
Collie's transmutation experiments, 207 ff.
Colloidal solations, 84
—— ——, Brownian movement in, 93
—— ——, catalytic action of, 90
—— ——, nature of, 85, 89
—— ——, poisoning of, 90
Colloids, 82 ff.
—— and crystalloids, difference between, 83
Complements, 69
——, non-selective character of, 71
Compounds, spectra of, 107
Contact process of sulphuric acid manufacture, 30, 91
Copper extracted from iron pyrites, 258
—— lithium transmutation, 204 ff.
——, spectrum of, 103, and Plate I
Corpuscles of blood, 51
Cosmic Evolution, 198
Crookes, 149, 159 ff.
Crystalline structure, 122–3
Crystalloids and colloids, difference between, 83
Crystals, liquid, 19
Curie, 193
——, Madame, 168, 206
Cyanide process for gold-extraction, 45–6

D

Darwin, 124
Davy, 292

Digitized by Google

Debierne, 193
Decay of radioactive emanations, 189
Dewar, 152
Dextro-compounds, 129
Diabetes, 33
Diagram of action of complements and intermediate bodies, 71
—— of anti-toxin action, 64
—— of agglutination process, 67
Dialyzer, 82
Diamond, 4
Diphtheria, 56
Disease, chemical study of, 33
Disintegration hypothesis, 182
Döbereiner, 138
Drugs, asymmetric, 129
——, colloidal, 96
Dust-counter, 188
Dyes, artificially synthesized, 11
——, used in coping with bacilli, 78
Dynamics, chemical, 14

E

Economic conditions and chemical problems, 242, 258, 259
Education grants, elementary and advanced, 291
Ehrlich, 60 ff.
Ehrlich's side-chain theory, 61 ff.
Electric arcs, temperatures and spectra of, 104
—— battery, 18
—— discharges in high vacua, 159
—— furnace, 37
Electricity and transmutation, 207
——, positive, 220
Electronic theory of chemical action, 221–2
—— —— —— matter, 221 ff.
Electrons, 164, 220
Electroscope, 167
Elements, characteristic spectra of, 107
——, criteria of nature of, 214
——, detection of new, by means of spectroscope, 103
——, dissociation of, 106
——, evolution of, 198 ff.
——, halogen group of, 139 ff.
——, inert, 135 ff.
——, nature of, 213 ff.
——, Periodic arrangement of, 137 ff., 155
——, prediction of unknown, 139 ff.
——, properties of, merely averages, 223 ff.
Emanation of radium (*see* Niton), 189 ff.
—— of thorium, 187 ff.
Emanations, radioactive, measurement of decay of, 189
Emulsin, resemblance between it and colloidal solutions, 90
Energy, 227 ff.
——, storage of, 231
Enzymes, 31, 32
Equilibrium, 14, 23
Evolution of elements, 198 ff.
Examinations, 271
Excited activity, 193
Explosives, as stores of energy, 232
Eye-measurements in science, 98

F

Faraday, 292
Fergusonite, 156
Fermentation, 33, 128
Fermentation, effect of asymmetric structure on, 128
Ferments, resemblances between colloidal solutions and, 90
——, result of injection of, into blood, 52 ff.
Fertilization by means of salts, 29
Fibrin, 52
Fischer, 244
Fixation of nitrogen, 240 ff.
Flames, spectra of and temperatures in, 104
Fluorine and its congeners, 140 ff.
Food supply, problem of, 233 ff.
Fraünhofer's lines, 112, 113
Fruit-sugar, 48
Furnace, electric, 37

G

Gamma-rays, 175
Gases, spectra of, under low pressure, 107
Gel, 84
——, conversion of, into sol, 85
Geological thermometer furnished by chemistry, 27
Geology and chemistry, 23
Germanium, Mendeléef's prediction of its properties, 142
Germany, appreciation of possibilities in coal-tar by, 12
Glass, stained, origin of colour in, 88
Glauber's salt, 27, 257
Gleditsch, Mdlle., 206
Gold, extraction of, 44 ff.
Graham, 82
Graphite, 4
Gray, 190, 191

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H

Half-life period of radio-elements, 184, 185
Halogen elements, 139 ff.
Haptophore group, 65, 69
Heat, specific, 17
Heat-rays in spectrum, 99
Helium, discovery of, 148, 149
Helium, evolution of, from niton, 195
—— in cathode ray bombardment, 210
—— in old X-ray bulbs, 211
——, origin of, in mineral springs, 195
——, properties of, 156
——, spectrum of, 102, 103, and Plate I
Hemihedral crystals, 123
Hertz waves, 99
Hillebrand, 147
Hofmann, 12
Holohedral crystals, 122, 123
Hydrochloric acid in Leblanc process, 259
Hydrogen, liquid, 152
—— peroxide, decomposition of, 90
Huxley, 292

I

Illuminating gas, 10
Immunity, 51 ff.
——, machinery of, on Ehrlich's theory, 72
——, Metchnikoff's theory of, 73
——, natural and acquired, 59
Immunization, active and passive, 59
Incandescent mantle, history of, 42, 43
Indigo, artificial synthesis of, 9
Intermediate bodies, specific character of, 71
Inert elements, place of, in Periodic Table, 149, 155
—— gases, 135 ff.
—— ——, niton a member of the series, 191, 192
—— ——, properties of, 154 ff.
—— ——, proportions of, present in atmosphere, 154
—— ——, spectra of, 156, 157
Inorganic chemistry, 2, 3
—— ——, methods of research in, 3
Isomeric compounds, 118
Intermediate bodies, 70
Invisible light, 100
Iodine and its congeners, 140 ff.
Ionization of gases, 166

J

Janssen, 1

K

Kieserite, 24
Kirchhoff, 113
Krypton, discovery of, 151, 152

L

Lævo-compounds, 129
Law of Mass Action, 16
Le Bel, 124
Leblanc process of alkali manufacture, 257 ff.
Lenard, 161
Lethal dose, minimum, 54
Leverrier, 124
Life and chemistry, 245, 246
Light-vibrations, 99
Liquefaction of gases, 150
Liquid crystals, 19
—— hydrogen, 152
Lithium from copper, 204 ff.
—— spectrum, 107
Living material, synthesis of, 245, 246
Lockyer, 105, 149
Loeb's experiments on fertilization, 29

M

Madsen, 73
Magnetic field, its effect on spectra, 218
Mantle, incandescent, radioactivity of, 171
Manure problem, 237 ff.
Manures, artificial, 238 ff.
Mass Action, Law of, 16
Masson, 209
Matter, unitary theory of, 215
Meldola, 284, 287
Mendeléef, 138 ff.
Mercury, drugs derived from, 79
Metallography, 19
Metalloids, 3
Metals and non-metals, 3
Metchnikoff, 60
Metchnikoff's view of immunity, 73
Meyer, Lothar, 138
Microbalance, 191, 192
Mineralogical chemistry, 19
Mineralogy, relation to colloid chemistry, 95
Mineral springs, helium in, 195
Mirror-image compounds, 124
Mixtures, spectra of, 103, and Plate I
Moissan, 38
Molecular weight, determination of, 254

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Monazite sand, 43
Movement, Brownian, 92 ff.
Muspratt, 258

N

Nebulæ, composition of, 199 ff.
Neon, discovery of, 152
—— from niton, 203
—— in old X-ray bulbs, 211
—— produced in cathode ray, 207 ff.
——, properties of, 157
Newlands, 138
Nickel carbonyl, 40
——, extraction of, from ore, 40
Niton, 186, 187 ff.
—— action of, on water, 192
——, atomic weight of, 191
—— decomposition of, 203
——, discovery of, 186 ff.
——, helium evolved by, 195
——, one of the inert elements, 191, 192
——, properties of, 190
——, spectrum of, 192
——, volume of obtainable, 189, 190
Nitrifying and denitrifying bacteria, 235
Nitrogen compounds, use of, as manures, 237
—— cycle in nature, 235 ff.
——, "fixation" of, 240 ff.
——, importance of, in food problem, 234
——, wastage of combined, 242, 243
Non-metallic and metallic elements, 3

O

Oil of vitriol, 30
Olszewski, 152
Opsonins, 74
Optical rotatory power, 122
Organic chemistry, 2, 5
—— ——, methods of research in, 8
—— ——, syntheses in, 8
Osmotic pressure, 28
—— —— and fertilization, 29
Owens, 187
Oxy-acetylene blowpipe, 39

P

Particle, cathode charge on, 163
——, ——, relation between charge and mass of, 163
Particles in colloidal solutions, Brownian movement of, 93
—— —— —— ——, methods of counting, 87
—— —— —— —— estimating mass of, 92
—— —— —— ——, size of, 87
Pasteur, 36, 122, 123, 125, 265, 266
Pathology and chemistry, 33
Patterson, H. S., 209, 212
Penicillium glaucum, 128
Periodic arrangement of elements, 137 ff., 155
Periodic Table, 139 ff., 155
—— —— and radio-elements, 185
—— ——, anomalies in, 155, 156
Perkin, Sir W. H., his work on the coal-tar dyes, 11
Phagocytes, destruction of bacilli by, 73
Phosphorescence, Becquerel's researches on, 166
Phosphorus, red and yellow varieties, 4
Photochemistry, 19
Photographic plates, colloidal structure in, 97
Photographic spectrum, 100
Physical chemistry, 13
—— instruments used in chemistry, 16 ff.
—— properties as criteria of purity, 252
Physics and chemistry, common ground between, 13, 22
Physiology and chemistry, 28
Phytochemistry, 19
Pitchblende, 168
Platinum as a catalyst, 30
Playfair, 277
Polarimeter, 17
Polarization of light, 120
Polarized light, action of, on certain asymmetric compounds, 133
Polonium, 171
Polyhalite, 24
Positive electricity, 220
Power, distribution of, 229–30
—— sources of, 227 ff.
Preparation, No. 606...80
Pressure, osmotic, 28
Proteins, 243–5
Protoplasm, synthesis of, 245
Protyle, 217
Prout's hypothesis, 216
Purification of chemicals, 249–51

Q

Quartz, its effect on polarized light, 121

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R

Racemic acid, 122 ff.
Radiant matter, 160–1
Radioactive elements as sources of energy, 233
—— emanations, breakdown of, 194 ff.
—— substances and the Periodic Table, 185
Radioactivity, measurement of, 167
Radio-lead, 171
Radium, 159 ff.
—— and medicine, 179
—— bromide, 169
—— cause of minerals discolouring when exposed to, 96
——, decay of, 182 ff.
——, energy of, 179
——, evolution of niton from, 189
——, origin of, 184
——, properties of metal and its salts, 169 ff.
—— salts, their action on water, 169
—— series, chain of descent in, 185
Ramsay, 203, 204, 206, 211, 212, 145 ff., 190, 191, 194, 195
——, suggestions of new elements in air, 145
Rare earths, 41 ff.
Rayleigh, 144, 145, 147
Rayleigh's experiments with nitrogen, 144
Rays, α, 175, 176
——, β, 175, 176
——, γ, 175
——, Becquerel, 167 ff.
——, cathode, 160 ff.
——, cathode, nature of, 162
Receptors, 61, 69
Refractometer, 17
Research, applied, 256
——, capacity of Britain, scheme for better utilization of, 293 ff.
—— factors adverse to, 287 ff.
—— grants, 282
——, methods of chemical, 3, 8, 248 ff.
—— work and examinations, 271
——, organization of chemical, 263, 268 ff.
—— Professors, 296
—— Scholarships and Fellowships, 274 ff.
——, School of, 264
——, training in, 266
—— work, methods employed in, 3, 8
—— ——, types of, 263–4
—— worker, career of, 265
Right-handed and left-handed forms of crystals, 123
Rock-formation, 23
Rock-salt, 23 ff.
Röntgen, 165
—— rays, nature of, 165
Rotatory power, optical, 122
Royal Commission for the 1851 Exhibition, 274, 277 ff., 304
Rubber, a typical colloid, 97
Rutherford, 182, 188, 191, 193

S

Salt-cake, 257
Salvarsan, 80
Scholarships and Fellowships, Research, 274 ff.
Sciences, ramification of, at present day, 1
Sea-urchin's eggs, fertilization of, 29
Seebeck, 122
Selective action in immunity, 55
Sera, normal and immune, 68
Serum, 52
—— therapy, 77
Sewage, wastage of, 238
Side-chain Theory of immunity, 61 ff.
Side-chains, 61
——, general and specific actions of, 61
Silicon carbide, 39
Sleeping sickness, 80
Snake venom, 53
Soap industry, 262
Soda-water, 39
Soddy, 191, 194, 195
Sodium sulphate, 257
—— vapour, absorption spectrum of, 112
—— —— spectrum of, 102, 107, and Plate I
Sol, 84
——, conversion of, into gel, 84
Solar prominences, 114
—— spectrum, 113
Solids, spectra of, 101
Solutions and suspensions, 89
Solvay process of alkali manufacture, 260
Spark spectra, characteristics of, 106
Specialism, 20, 21
Specific heat, 17
Spectra, absorption, 101, 108 ff.
——, characteristic of elements, 107
——, continuous, 101, 104
——, effect of magnetic field on, 218
——, —— —— temperature on, 104
——, emission, 101
——, evidence of Zeeman effect in, 218–221

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Spectra, influence of temperature on, 104 ff.
——, line, 101, 104
—— of arcs, 104–5
—— of compounds, 107
—— —— electric sparks, 106
—— —— elements, 217
—— —— flames, 104
—— —— incandescent solids, 101
—— —— inert gases, 156–7
—— —— vapours, 101
Spectral series, 218
Spectroscope, 18, 22, 98 ff.
—— applied to stars, 198 ff.
——, discovery of new elements by means of, 103, 148 ff.
——, its sensitiveness, 103
——, —— use in analysis, 103
——, —— —— —— astronomy, 112 ff.
Spectroscopic discovery of helium, 148
Spectrum, electrical, 100
—— of acetylene lamp, 101, 102, and Plate I
—— —— carbon arc, 101, 102, and Plate I
—— —— —— vapour, 102
—— —— copper, 103, and Plate I
—— —— helium, 102, 103, and Plate I
—— —— lightning, 192
—— —— niton, 192
—— —— sodium vapour, 102, 107, and Plate I
——, photographic, 100
——, waves making up, 99
Spinthariscope, 176
Spirochætes pallida, 79
Stars, classification of, 200
——, composition of, 198 ff.
——, spectroscopic study of, 115 ff.
Stassfurt deposits, 24 ff.
—— salts, commercial exploitation of, 46
Statics, chemical, 13
Statistics of alkali industry, 260
—— —— Carnegie Trust, 275–6
—— —— Chemical Society's research grants, 282
—— —— Chili nitre beds, 237–8
—— —— 1851 Exhibition Scholars, 279–80
Stellar evolution, Lockyer on, 201
Stereochemistry, 17, 118 ff.
Structure, chemical, 119
Sugar extraction, processes employed in, 48 ff.
Sugars, 33, 48
——, fermentation of, 33
——, reducing, 48
Sulphur dioxide, 30
Sulphur in Leblanc process, 258
—— trioxide, 30
Sulphuric acid, contact process of manufacture, 30, 91
—— —— manufacture, 258–9
Suspensions, 89
Synthetic foods, 244–5
—— work in inorganic chemistry, 8
Syphilis, 78

T

Table of germanium's properties as predicted by Mendeléef, 142
—— —— wave-lengths, 100
Tartaric acid, 122 ff.
Taste, influence of space factors on, 129
Temperature and transmutation, 203
——, its influence on spectra, 104 ff.
Temperatures of electric arcs, 104
—— —— electric sparks, 106
—— —— flames, 104
Tetrahedral groupings, 125–6
Thermodynamics, 19
Thermometer, chemico-geological, 27
Thorianite, 43
Thorium, degradation of, 182
—— oxide, 40
Tourmaline, 120
Toxins, 54 ff.
Toxophore group, 65
Transmutation, 197 ff.
—— and electricity, 207
—— —— temperature, 203
Travers, 152
Triboluminescence, 177
Typhoid fever, 56, 64

U

Ultra-microscope, 86
United Alkali Company, 261
Uranium, 168
Uranium-X, 180
Urea, Wöhler's researches on, 2
Usher, 203, 206

V

Vaccination, 51, 60
Van 't Hoff, 27, 124, 266
Vapours, spectra of, 101
Vesuvius, 156
Vital action and chemistry, 133, 245–6

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W

Wallace, 124
Wave-lengths of spectrum, 99
Weight, atomic, 138
Welsbach, 42
—— mantle, radioactivity of, 171
Widal's reaction, 65
Wilson's dust-counter, 188
Wind-power, 228 ff.
Wöhler, researches on urea, 2
Wright, 74

X

X-rays, nature of, 165
X-rays and Becquerel rays, 177–8
Xenon, discovery of, 153

Y

Yeast, 31

Z

Zeemann effect, 218–221

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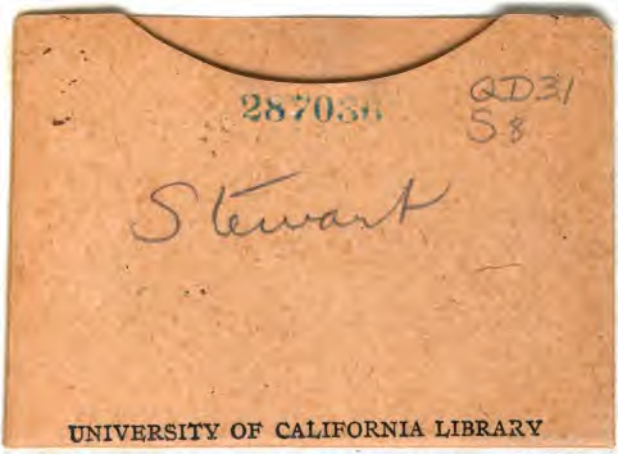

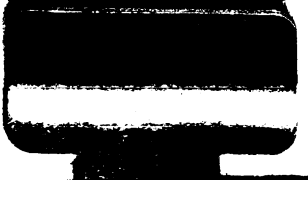

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